The Classical Review

JUNE 1899.

The Editor of the Classical Review will be glad to receive short paragraphs (or materials for such paragraphs) upon classical topics of current interest. These should reach him as early as possible in the month preceding the publication of the REVIEW.

Under the somewhat ambitious title, 'A Classical Discovery,' the Cambridge Magazine of May 25 publishes a paper by Mr. G. F. Abbott, an undergraduate of Emmanuel College, upon a coin of Septimius Severus which was discovered in Egypt in 1894. Mr. Abbott uses the coin, of which he gives a photograph, and which represents soldiers passing over a bridge, to defend the statement of Eutropius and Victor that Severus ascended the throne after defeating Julianus in a battle on the Mulvian bridge against the received account of Dio Cassius and Herodian. Mr. Abbott does not appear to to have convinced the distinguished scholars whose letters he prints; but the publication of his paper in an undergraduate periodical is a very encouraging sign. It is a pity that the whereabouts of the coin itself is apparently not known.

Apropos of the heavy indictment which Mr. Sloman prefers in another column against the 'Revised Latin Primer,' a wellinformed correspondent communicates the following remarks :-

'The monopoly of the Revised Latin Primer has remained unassailed since the meeting of the Head Masters' Conference at Charterhouse in 1885, when Mr. Young's motion in favour of free trade in Latin grammar was rejected. On that occasion, many votes were doubtless influenced by a personal appeal of Dr. Kennedy, who said that he wrote the Primer at the invitation of head masters, and on their express promise to use it exclusively; that he had

resigned the headmastership of Shrewsbury on the faith of that promise, and that the book was the main provision for his family. At the same time he circulated proofs of the revised edition, and asked for corrections and suggestions. But the book appears to be really incorrigible, and it is time that the privilege which it has enjoyed for more than thirty years should be withdrawn. There is probably no headmaster now in the profession who was a party to the original agreement with Dr. Kennedy.'

The last intelligence concerning the recently-discovered MS. of the Agricola contained in the letter of Mr. Furneaux which we print elsewhere, will be received by our readers with astonishment and dismay. One might have thought that in the year 1899 such an incident was impossible even in Spain. If the Bishop of Toledo can only take the commercial view of learning, let him sell the manuscript of which he is no fit custodian.

A correspondent suggests that the Classical Review should publish 'Scholarship' papers together with hints for answering them, like those in Gantillon's well-known collection. There are difficulties in the way of adopting the proposal, at least in this particular form. But we take advantage of its being put forward to say that we shall always welcome any suggestion with which correspondents may favour us for making the Classical Review more efficient or popular.

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HANNIBAL'S ROUTE OVER THE ALPS.

THE printing of anything more upon this subject seems to me to need explanation. In fact this article, save for a few allusions to the researches of more recent writers, especially those of Dr. Fuchs,1 was written, as it now stands, more than a dozen years ago, for a private society, without any idea of publication. I thought then that Mr. Douglas Freshfield's article, published in the Alpine Journal of 1883, and accepted by Mr. W. Arnold in his edition of his grandfather's chapters on the Punic wars, had finally settled the question for the main part of the route. But I find now that his really illuminating treatise is apparently ignored by many scholars, and by many subsequent writers on Roman history. Messrs. How and Leigh indeed adopt the right view, but Mr. Shuckburgh retains the old theory, and, still more recently, Mr. Greenidge in his excellent revision of Smith's Smaller History of Rome, published in 1897, supports the old case by arguments which had been entirely overthrown by those of Mr. Freshfield. Lastly Dr Fuchs, who has already written with ability on the military aspects of the Second Punic War, though in a learned and careful treatise on the Pass of Hannibal he arrives at what I believe to be the right conclusion, does not seem to me to grasp the relative importance of the arguments for it, and against other conclusions, or to have discovered all of them. It is strange that with all his industrious research he never alludes to Mr. Freshfield's treatise at all. For him 'die Engländer' and 'die englischen Gelehrten,' often referred to, are Cramer and Wickham, who wrote about eighty years ago.2 For all which reasons I think it will be not altogether useless to put forward a statement of the case-indeed I was urged to do so by a friend whose authority on Roman history stands very much higher than my own.

In studying the question I have been struck by one or two phases of contemporary writing. One is the unceremonious manner in which any ancient writer (and the same may be said about many ancient MSS.)—any ancient writer who traverses

the course selected by the modern essayist is swept aside by the most fanciful arguments, as if he were no obstacle at all. Even Thucydides is now told that in his narrative of the sieges of his own time he purposely embroiders. In reading such essays one is reminded of his own phrase ' ἀγωνίσματα ἐς τὸ παραχρῆμα.' It is surely time to protest against the zeal for rewriting history, and also against the modern method of criticising through a microscope, which shuts off a view of the surroundings. It is the fashion so to deal with Livy. We are told that he need not be reckoned, because he only cared to write picturesquely, We may alter the names of places; admire cum risu his descriptions; when he says 'ad laevam' we may, with one commentator, say he means 'to the right,' and when he says 'cis Volturnum' we may, with another, assert that he meant on the further side of the river. Now I believe that this is in great measure nineteenth century arrogance, and to some extent nineteenth century haste and impatience. I feel sure that many are far too ready to discount Livy's evidence; but at any rate, if he did not always use his materials critically, he used them honestly, and he used them more critically than is sometimes supposed. On this particular question it is pretty clear that he had the narrative of Polybius before him (though some have denied it); and he tells us himself that he was using the account written by Cincius Alimentus the annalist, who had been a prisoner in Hannibal's army at one time, and was more likely to know the details of the campaign than even the most recent critics of Livy.

Another phase, as I think, of this age, is the tendency of people to accept and repeat certain statements which they do not themselves understand, or to judge of matters of which they have no experience from their own preconceived notions. Even so great a man as Mommsen is hasty and unsound in his reasoning (whether right or not in his faith) upon this matter: no doubt, because he did not, and justly did not, consider this question of vital importance to the history. He says: 'The pass of the Little St. Bernard, while not the lowest of all the natural passes of the Alps, is by far the easiest; although no artificial road was constructed there, an Austrian army with artillery crossed by that route in 1815.' (I

¹ Hannibals Alpenübergang, von Josef Fuchs, Wien,

bei C. Konegen, 1897.

² See also p. 10, 'Seit Mommsen diese in England herrschende Partei in seine mächtige Patronanz genommen hat...,' &c,

quote Mr. Dickson's translation.) Now as a matter of fact the Little St. Bernard is naturally steeper and more difficult ground than either the Genèvre or the Col de l'Argentière, and as to the argument of the Austrian army in 1815, it might as reasonably be made an argument for the Genèvre that Charles VIII. crossed it in 1474, or for the Argentière that Francis I. crossed it in 1515. Neither pass then had a carriage road, which I suppose is what he means by 'an artificial road.' There were Roman roads of some date or other on all three. A little further on he adopts with approval the argument offered by two English travellers that they found snow on the slopes of the Little St. Bernard at the end of August, and upon it makes the absolutely erroneous assertion-'on the Little St. Bernard winter begins about Michaelmas and the falling of snow in September.' have been in deep snow at that height in August; but it was merely from a chance and exceptional snowfall lasting only for a Winter, that is, snow which remains, cannot be said now, as a rule, to begin before the second week in October, and the conditions are much the same for all the competing passes: whatever may be said about the snow encountered by Hannibal, it cannot be made a means of determining the pass. Mommsen's statement that the Carthaginian army 'had no choice' would be blameworthy if the matter had real historic importance, and even here startles us when it comes from the great German his-

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Mr. Bosworth Smith adds another proof: 'the reported' discovery of elephants' bones on the pass in 1769. I have not been able to discover the report, but if it really was reported, I should like to know where the bones are, and who examined them. I have read a book on the geology of Auvergne which mentions that just about the same time the discovery of the remains of an oyster feast of a former mayor of Clermont Ferraud started the theory that the Limagne was a marine, and not, as it really is, a fresh water deposit. And then Mr. Bosworth Smith goes on to speak of Hannibal on a crevassed glacier, thereby putting the glacial period of such places, say, as the Tête Noire within 2,000 years of our own time. The fact is, neither Polybius nor Livy use any words which imply either glacier or crevasses. Yet once more-Mr. Capes in his very excellent and

1 I see that Dr. Fuchs, p. 97, mentions this report and justly sets it aside.

useful edition, censures the luckless Livy for his description of the country approaching Col Bayard-i.e. between the Isère and the Durance-' visa montium altitudo, nivesque caelo prope immixtae, tecta informia imposita rupibus, omnia rigentia gelu, cetera visu quam dictu foediora'—all which things terrorem renovant.' On which he writes, 'The following description is somewhat absurd applied to the lower Alpine valleys and is a bit of fine writing, in which Livy gives full scope to his rhetorical taste, working upon the description of some traveller who exaggerated the horrors of his journey.' I appeal to any Alpine traveller, whether it is not just these rugged summits in the approaches to the great mountains which look most hopeless and heartbreaking, if one is bound to climb them? The mountains 8,000 or 9,000 feet high with châlets perched two-thirds of the way up, which one is disposed roughly to estimate on a level with the moon? And in this district the unlucky army, by no means out for its holiday, would see these hills of 8,000 feet backed by the Dauphine giants-the Ecrins, of which Mr. Whymper (perhaps a little underrating ideal misery) writes: 'I may put it on record as my belief that however sad and miserable a man may have been, if he is found on the summit of the Pte des Écrins after a fall of new snow, he is likely to experience misery far deeper than anything with which he has hitherto been acquainted '-and with the Écrins, the yet more forbidding Meije standing, as it happens, exactly where a bee line to their pass, if it was the Genèvre, would take them. Is Livy then too rhetorical when he speaks of amazement and terror? And then the same writer who has so reproved Livy's rhetoric, brings an argument for the Little St. Bernard, 'that it is steeper on the Italian side, which agrees with Polybius's account.' It is a commonplace in books of Physical Geography, and is perfectly true, that the southern side of the Alps is nearly everywhere steeper, the mountains being to some degree what Mr. Leslie Stephen calls 'of the writing-desk pattern,' with the gentler slope on the north side.

I think it may somewhat clear the way if I set down a few preliminary considerations in the form of resolutions, which may be mentally passed or rejected.

(1) Livy, who says that he studied the question, and had before him the writings of a Roman who had been prisoner in Italy with Hannibal's army, is not likely to be wholly mistaken as to the line of country followed, especially on the Italian side. Therefore, if we follow most readily Polybius, who tells us that he conversed with men who lived through the events, and that he himself travelled over the actual route, still it is desirable to induce Polybius and Livy to go the same way, if we can do so without doing any violence to the Latin or

the Greek language.

(2) Exactness in miles or stades is not to be looked for in Polybius. No one can suppose that in his day the route was marked by mile-stones; and any one who has travelled in the Alps knows that, off a driving road, distance is reckoned by hours, not by kilometres. Whereas then Polybius gives 10 days, 100 Roman miles to the ἀναβολὴ τῶν 'Αλπέων and then 15 days 150 Roman miles to the Italian plains, are we not warranted in suspecting the coincidence and concluding that mileage goes for nothing and that he knows the number of days and assigns roughly 10 miles a day? 1

(3) His account of days must be taken as authentic, and the distance possible or impossible for infantry to accomplish in a given time must affect the question.

(4) It cannot be assumed that Celtic tribes had precisely the same limits of territory in 218 B.c. and in the time of Julius Caesar.

(5) All Alpine valleys of any length contain gorges more or less rocky, white rocks, whether from their relatively white formation or because they have been recently exposed by some landslip, places where Gallic towns did stand or might have stood, and places where avalanche beds might remain and receive a fresh coating of snow in September or October, or where a slope might be coated with two distinct falls of snow. Therefore minute local correspondences are an *ignis fatuus*—what may be predicated in this way of one pass may with equal confidence be predicated of another.

The ancient authorities whom we have as our guides are, (i) Polybius, who is careful to assert his topographical knowledge. 'I write,' he says, 'with confidence, having not only enquired from men who lived at the time of these events, but having also my-elf seen the places of which I speak and made the passage of the Alps as an enquirer and a sight-seer '—γνώσεως ἔνεκα καὶ θέας—excellent reasons for travel, and rare at that time.

(ii) Livy of whose materials in the writings of Alimentus I have already spoken, and who certainly shows careful geographical study in his examination and rejection of the Val d'Aosta route.—The accounts of Livy and Polybius will have to be considered more in detail.

(iii) Nepos who says that 'H. crossed those Alps which separate Italy from Gaul, which nobody ever crossed before with an army except the Greek Hercules, for which reason this pass is called Graius.' He would be an unscrupulous man who asserted that this limits us to the Graian Alps, since it is impossible to say whether Nepos means that Hannibal crossed in the same place as Hercules—nor is it worth while to enquire, for the whole statement is confused. Hercules's army consisted of cows, and Polybius tells us, what we might guess, that the Celts before Hannibal's coming had 'often' (οὐχ ἄπαξ οὐδὲ δίς) crossed the Alps

with large armies. (iv) We have Varro, not merely the most prolific, but also the most learned writer of his age, whom Cicero calls 'diligentissimus inuestigator antiquitatis'; who was born only a few years after the death of Polybius; who as Pompey's lieutenant had crossed the Alps, and who must have been the great literary authority at Rome when Livy first became known in Roman society. Varro gives this very important opinionquoted by Servius (ad Aen. x. 13): 'The Alps can be crossed by five passes—one which is near the sea through the Ligurians: the second by which Hannibal crossed: the third by which Pompey went to the Spanish War: the fourth by which Hasdrubal came from Gaul into Italy: the fifth which was formerly occupied by the Greeks, and thence comes the name Alpes Graiae.'

5. Strabo, who came to Rome as a young man the year before Varro died, and died himself not long after Livy, quotes Polybius (the passage is not otherwise preserved) in these words: 'Polybius names only four passes of the Alps: through the Ligurian territory next the sea; then through the Taurini, which Hannibal crossed; then through the Salassi; fourthly, through the Raeti' (Strab. iv. p. 209).

It is true that in one MS. but only one, the words $\hat{\eta}\nu$ ' $\Lambda\nu\nu(\hat{\beta}\alpha_S, \delta\iota\hat{\eta}\lambda\theta_E)$ are omitted, and some critics may therefore declare them to be interpolated. I see no reason for rejecting them; but at any rate the suggestion in an excellent edition of Livy is amusing—that they might have been transposed, i.e. you may, if you like, assume that Strabo really

¹ This is correctly perceived by Dr. Fuchs, p. 103.

wrote, not, as we find it, 'through the Taurini where Hannibal passed,' but 'through the Salassi where Hannibal passed.'

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First, reckoning from the north, the Little St. Bernard in the Graian Alps, which, quitting the Isère at Bourg St. Maurice, turns N.W. up the torrent Récluse and comes down into the Valley of Aosta (the territory of the Salassi).

2. Mont Cenis, or the Little Mt. Cenis, in the Cottian Alps, which leaves the Isère, turning S.E. by the Valley of the Arc, and comes down to Susa—that is to say, keeping east and north of the tunnel and railway.

3. Mont Genèvre, also in the Cottian Alps, which leaves the Isère by the Valley of the Drac, reaches the Durance (Druentia) near Gap, and passes by Briançon to Cèsanne and Susa.

4. The Col d'Argentière. This pass is the same as the Genèvre as far as Chorges between Gap and Embrun. It there leaves the Durance going S.E. up the Valley of the Ubaye by Barcelonette, and down to Cuneo.

It is not necessary to consider such wild ideas as the Great St. Bernard, which would have taken him past the Lake of Geneva into the Upper Rhone Valley-or the high passes near Mt. Viso; still less the Simplon or the St. Gothard, which have both, absurd as it seems, found advocates.1 I should notice however a passage of Arnold, whose sense of geography led him to distrust the Val d'Aosta route. He says (Vol. iii. Note M.) 'I have often wished to examine the pass which goes by the actual head of the Isère by M. Iséran and descends to Chivasso.' He From would have examined only to reject. the head waters of the Isère, in the Val de Tignes, the Col de Galise leads to Ceresole and Chivasso. It is a glacier pass nearly 10,000 feet high, presenting no great difficulties to persons accustomed to mountains, and provided against emergencies with axe and rope—but that is all that can be said for it. It passes, as Arnold conjectures, close to that impostor Mt. Isèran, in Arnold's time still believed to be a high mountain.

It may be interesting to note opinions expressed by Napoleon, who often dwelt upon Hannibal's passage, and sometimes, though apparently not always, believed him to have crossed the Genèvre. In that most interesting of the many books about him, Ségur's memoirs, it is said that on his march

by the maritime Alps in 1796 he pointed towards Mte. Viso, and said to his staff 'That is where Hannibal passed.' His staff seem to have thought that he meant over the Viso or over its shoulder; for Ségur adds (I only quote from memory) 'We observed that that was not the usual belief.' Hennebert seems (p. 50) to have been misled in the same way; but it should be noticed that the direction in which Napoleon pointed would do for the Genèvre as well as for unlikely passes such as the Traversette. More important, however, as an authority is the saying of Napoleon, which Hennebert rightly thinks valuable. In rejecting that Little St. Bernard route on one occasion, he said 'il ne l'a pas fait, parceque le texte de Polybe et de Tite Live est positiv, parcequ'il n'a pas du le faire.' The military judgment underlined is worth noting.

The conditions to be fulfilled may be gathered from the histories as follows, taking Polybius as the chief authority, and noting whether or not he is really at

variance with Livy.

Hannibal in four days' march from the Passage of the Rhone between Orange and Tarascon reaches what is called 'the Island,' formed by the Isère joining the The great triangle, so cut off, Polybius compares to the Delta of Egypt, saying that the base, instead of being sea as in Egypt, is formed by the mountainsclearly the range of Mont du Chat. His object was to settle some dispute between two factions of the natives, and to get supplies and guides from his friends.² He then marches 10 days along the river (παρὰ τον ποταμόν), 800 stades = 100 Roman miles or 92 English miles, before he reaches the 'ascent' (ἀναβολή) to the Alps—that is, I conceive, before he faces a steep ascent, either leaving the valley in which he has been hitherto marching, or else entering upon a part of the same valley distinctly steeper and more rugged in character. So far he has had easier ground, in which his cavalry could act enough to deter the Gallic attack. It is not necessary to postulate a plain like the Valley of the Thames. He has also been conducted by certain barbarians belonging to the chief whom he had supported on the Island. According to Livy the inhabitants are Allobroges, though Polybius does not say so. It is noticeable

¹ See Hennebert, Hist. d'Annib. ii. p. 46.

² Hennebert tries to account for his not striking up the Durance from its confluence with the Rhone by the fact that the Salyes, on the lower Durance, were hostile. His business in 'the island' is a simpler explanation.

that Polybius never names the river, and in fact gives no local names in any part of the subsequent route. Livy tells us that in leaving the island—i.e. as I take it, from Valence—he bore to the left through the Tricastini. The Tricastini lived between the Isère and the Drôme. Thence, as Livy says, passing the Vocontii (who lived about Vizille) and the Tricorii (who lived near St. Bonnet) he came to the Druentia (Durance) by a country which presented no difficulty. This march without difficulty should cover the ten days which Polybius mentions as

being easy and little molested.

At this point he is attacked by some Allobroges (so named by Polybius as well as by Livy) in steep and difficult ground. He forces what is apparently a pass or defile with a town in more open ground beyond, which he occupies. There he rests a day and gathers provisions. Livy particularly mentions that the country just after this difficult place was more populous and better cultivated. After marching six days he is attacked in a defile which Polybius calls φάραγγα δύσβατον καὶ κρημνώδη, and Livy angustiorem viam parte altera subjectam jugo imminenti.' The cavalry got through to more open ground, but he himself with the infantry halted for the night under arms by 'a strong (or defensible) white rock.' On the next day—the ninth day as Livy says from the beginning of the ascent-(and this agrees with the reckoning of Polybius) he reached the summit of the pass. His soldiers are encouraged by the view, real or presented to their imagination, of the Italian plains. He remains two days and begins the descent, coming to some very difficult ground owing to the breaking away of the path, and complicated by old and fresh snow lying on the slopes. This place is only a short way below the summit and we are told that when he has conquered the difficulty, he reaches the plains within three days from that spot (τριταΐος ἀπὸ τῶν κρημνῶν), having marched, according to Polybius 150 miles (141 English) from the ascent to the plains in fifteen days. He descended into the plain of the Po: for Polybius says 'crossing the Alps he was to come to the plains of Italy which are about the Po,' and afterwards he says 'Hannibal descended with confidence (τολμηρῶς) into the plain of the Po and the country of the Insubrians'-it is mentioned however, that he encamps to rest his troops 'under the very skirts of the Alps,' and after this rest attacks the Taurini who dwell next the foot of the mountains. Livy states that the first people he came among were

the Taurini, which 'inter omnes constat.' I shall consider presently whether there is any real conflict between Livy and Polybius.

Of one point only in this question I feel quite certain, that, whether the Genèvre is found to be the pass or not, the Little St. Bernard must be unhesitatingly rejected, though in modern histories it has far the

greatest number of votes.

My objections are, that it is positively rejected, as all admit, by Livy, and in my opinion, also by Varro and Strabo; and that, even if we look to Polybius alone, and throw Livy overboard, we shall find that it does not satisfy his conditions. First, as regards the western side. The distance supposed to be required from Valence to what Polybius calls 'the ascent of the Alps' is made up by taking Hannibal up the Mt. du Chat-that very ridge which the historian himself calls almost inaccessible—(δυσπρόσοδα καὶ δυσέμβολα καὶ σχεδὸν ὡς εἰπεῖν ἀπρόσιτα), and the ascent and descent of which would be gratuitous, as it would only bring him to Chambéry and Montmélian, which he could have reached more easily by the valley of the Isère. As Mr. Freshfield points out, it is the sort of ascent which Polybius seems to condemn in chap. 47, when he calls those who suggest impracticable routes many bad names. For whether the ridge was pathless and impracticable or not, Polybius clearly thought that it was so.

It is true that there was a Roman road later over this ridge, continued from the Little St. Bernard, but that was to make a Roman route Roman fashion, as straight as possible through Aix, and on to Vienne and Lyons. I cannot believe that Hannibal would have dragged his army and elephants up and down that ridge 4,000 feet high; and, if he had, is it not strange that no mention is made of his coming down directly upon a lake 10 miles long (the Lac de Bourget) 11 Personally, as I have said, I think little of the mileage, and, if I wished to take Hannibal over the St. Bernard, I should take him from Valence by the Isère without touching the Mt. du Chat and Bourget; but I dwell upon the improbability, because the correspondence in distance to the ἀναβολή is used as one of the principle arguments for the pass. But is there this correspondence! It is measured, they say, παρά τον ποταμόν and this river they declare to be the Rhone and nothing but the Rhone. Therefore they measure 1,400 stades along the river to Mt. du Chat. But to measure this they leave

¹ Dr. Fuchs justly comments on this silence about the Lac de Bourget.

the great bend and cut across country, My view of 'along the river' is that Polybius meant thereby that they kept to a river valley until they reached the $d\nu a\beta o\lambda \dot{\eta}$: and whether the valley was the real Rhone valley or valleys branching from it, does not signify. The name of the river is not mentioned, nor do the words $\pi a\rho \dot{\alpha} \ \tau \dot{o} \nu \ \pi \sigma \tau a - \mu \dot{\phi} \nu$ occur in connexion with any mention of the Rhone.

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It appears to me that Dr. Fuchs wastes a great deal of time in trying to prove that Hannibal marched his army only up to the 'island' (i.e. the country between the Rhone and the Saone to the north of the Isère), and did not cross into it, merely drawing up his army along the south bank of the Isère outside the island. It does not matter, as it seems to me, whether he took his army into it or not. The point is that he did not march through the 'island' and over the Mt. du Chat; but if he did enter it, returned again across the Isère. I think, myself, that he did enter the 'island with his army, or some portion of it. His object was to settle a quarrel between the two brothers, chiefs of the Allobroges, and so out of this pacification to get the friendship of the brother whom he found it his best policy to support, to secure guides and supplies from the natives, and possibly safe conduct through tribes who lived further to the south. That he did all this by drawing up his troops on the other side of the Isère seems to me improbable. However that may be, Dr. Fuchs strains the Greek language in a manner which cannot be approved, when he tries by a long disquisition on words compounded with prepositions to show that συνεπιθέμενος must mean moral support only. So far from that being the case, I should say that the word can mean nothing but 'joining in an attack,' which would imply that he did take at least some of his

troops across. I fear I can only regard as the same sort of σκινδάλαμοι ἐπῶν his long discourse on Greek prepositions (pages 18-64). He takes a great deal of trouble to prove that ώς έπὶ τὰς πηγάς,' 'ώς εἰς τὴν μεσόγαιαν της Ευρώπης' imply 'in the direction of' etc. and do not merely = $\epsilon \pi i \tau \dot{\alpha} s \pi \eta \gamma \dot{\alpha} s$, etc. Of course it is so, but who in the world now would suppose that Hannibal went up to the glacier from which the Rhone issues,2 or that he reached the precise centre of Europe? But then he lays stress on the point that eis means 'into,' πρὸς 'to.' What then? Of course ηλθε πρὸς την νησον does not tell us that he went into the island; but it does not deny that he went into it. Hence we really gain nothing by a laboured discussion summed up (p. 34)— Also πρὸς drückt die Tendenz nach der Nähe, ἐπὶ nach der Begrenzung, els nach dem Innenraume eines Objectes aus.' It is all quite correct, but it proves nothing. He is not however correct in his contention that the words 'πρὸς ην ἀφικόμενος...καὶ καταλαβών ἐν αὐτη δύ άδελφούς, proves that he did not enter, and did not lead his army across.' I am totally unable to follow his argument based upon two pages of passages in which he finds the word $\kappa a \tau a \lambda a \mu \beta \acute{a} \nu \omega$ used. They are, I fear, as misleading as the five pages which follow with a collection of passages where συνεπιτί- $\theta \epsilon \sigma \theta a is used$. Much of this has not merely the detriment of criticism through a microscope. The error is not only that the significance of certain Greek words is unduly magnified; but some are not, I venture to think, correctly translated. I feel sure that Dr. Fuchs comes to a right conclusion in the end on the main question, and that many of his arguments are true and valuable. What I regret is, that he does not perceive their relative importance, and therefore produces much less conviction than he otherwise might produce in the mind of the reader. He does not seem, for instance, to grasp the fact that the really unassailable argument against the Little St. Bernard-not the only argument, but by far the strongest-is that the distance between the Col and the plains of the Po could not possibly be traversed in the time which Polybius allows. This point will be dealt with more fully below.

With the rest of the western side of this pass I am not concerned. All the distances and localities will do very well, and an

² It is true that Dr. Hoefer (1859) did suppose Hannibal to have passed the Rhone glacier and gone over the Furka and St. Gothard passes (see Hennebert, ii. p. 46): but it is waste of time to discuss such a theory.

excellent white rock is provided. But the really fatal objection to the Little St. Bernard, as the pass described by Polybius, is found on the Italian side-on that side which must have been best known to the historian. Hannibal is brought down into the Valley of Aosta—a valley hemmed in by high mountains: the great Pennine Chain on the left, the Cogne Mountains, a snowy range with the Grand Paradis over 13,000 ft. high, on the right. It is a valley, no doubt, but so little of a plain that any one can judge its character by a glance at the map—only three little narrow patches by the river side in all that 70 miles free from contour lines. Mr. Freshfield justly terms it 'a long defile.' It is not only that the great ranges send out spurs; there are also the huge moraines which make the roadway broken and intricate in many places. These moraines are respectable mountains. None within the valley attain the vast size of the old moraine of the (which is 2,000 feet high); but they form ground which in Wales or Scotland would be called mountainous. This valley so enclosed by mountains, and cut up by its own hills, narrows as one goes down it, and has its exit by the famous rocky gorge at the Fort de Bard. It is clear that Hannibal could not be said to have reached 'the plains about the Po,' until he passed out of this valley. But what are we asked to believe? The point where he issues from the Valley of Aosta is 82 English miles from the summit—at least 75 miles from the κρημνοί, where he is supposed to cut his road, and this distance of 75 miles of rugged ground he and his whole army have traversed before the end of the third day. It is hardly too much to say that this is impossible: it is also most improbable that, if he passed this valley at all, Polybius should make no mention of it, or of the warlike Salassi who inhabited it. Were they friendly? If so, why did he not halt to rest in the open ground about Aosta instead of pressing on to the plain of the Po? He does not mention them at all. And is it not strange that he should say nothing of the gorge of the Fort de Bard, where the only natural exit is monopolised by the river and the only roadway cut out of the rock? It stopped Napoleon in 1810 and nearly rendered his passage to the Great St. Bernard useless, or even disastrous. He marched his troops indeed over the mountains on the right bank of Dora, but it was by steep and difficult ground where his artillery could

not follow, and his guns were dragged through the street below the strangely unsuspicious fort, wrapped in straw. Of this remarkable gorge there is not a word in Polybius. I have no doubt that Livy is thinking of it and of the general character of the Valley of Aosta when he objects to the 'Pennine' (i.e. Great St. Bernard) route and to the Little St. Bernard also, on the ground that 'ambo saltus eum non in Taurinos sed per saltus montanos ad Libuos Gallos deduxissent.' This 'per saltus mon-tanos' Madvig alters to 'per alios montanos.' I think that the error lies in the first 'saltus.' It is possible that 'qui ambo saltus' has displaced 'quae ambo' (iuga). Why should Livy write 'alii montani' instead of giving the name? Anyhow the sense is plain. Livy says: 'If he crossed either of these passes, he would have to go through rugged defiles to get out of the Valley of Aosta to the Libui Galli' (who lived in the plain of the Po); and it is just these 'saltus montani' and still more the impossibility of a mixed army, and a tired army, marching 25 miles a day which hopelessly condemn the Little St. Bernard.

My reason for rejecting the Cenis is that if this route is chosen, all attempt to retain Livy's account must be abandoned. The road follows the Valley of the Arc, and the Druentia is never touched. The Cenis may no doubt be made to agree fairly with Polybius's description, but certainly not more

closely than the Genèvre.

If the little St. Bernard and the Mt. Cenis are rejected, there remain the Genèvre and the Argentière. The latter and more southern pass is on the whole preferred by Mr. Freshfield, whose article, as I mentioned above, I found convincing as regards most of the route, and more thorough than any other treatise that has dealt with the subject, though I differ from his conclusion as to the point where the main chain was crossed.

The Argentière branches off from the Genèvre by the valley of the Ubaye, and, striking far to the south, descends upon Cuneo. It is this ultimate goal which condemns it in my mind. Hannibal would have been brought to a point fifty-four miles south of Turin, and a great way south of the Po. This seems to me inconsistent with his subsequent operations. There is no mention of his crossing the Po in Polybius, but he seems to march at once upon the town of Taurini, at or about Turin. I cannot believe that he was ever so much to the south as Cuneo, until after the campaign of Trebia; nor that

ragged if he had been, we should have no mention either in Livy or Polybius of his passing ely un-Of this from the south to the north of the Po. Historically the pass of Argentière is interord in livy is esting because of the wonderful passage of Francis I., whose guns proved as difficult as aracter ects to elephants to manage, and were slung from rock to rock by ropes and pulleys. I regret) route to give up the parallel, which Jovius (quoted on the on in by Mr. Freshfield) evidently had in his mind; for he borrows Livy's actual words 'rupem Libuos s monferro pandunt' in relating Francis I.'s passage: but, as I said, I cannot believe that mon-Hannibal descended so far to the south. in the Left therefore with the Genèvre, we may i ambo trace Hannibal's route as follows. He leaves the 'Insula' by its southern edge (iuga). ni'inat Valence turns 'ad laevam,' that is, inow the crossed stead of marching due east to the Alps, which he faced, he bears to the left or e to go of the north of east, up the more level valley of ' (who the Isère. His march παρὰ τὸν ποταμὸν for is just ten days I take to mean merely that he folre the lowed a river valley so long before the asa tired cent to a pass came; not necessarily nor hopeactually the same river, as we name it, but an affluent. From Valence to Grenoble is is that about fifty miles, and the ordnance map retain shows on the right bank of the river a fairly The open space: and in that distance the, fall nd the of the river appears to be only 400 feet: In other words, it is not difficult ground, is may Polysuch as would have prevented his cavalry from acting, as described, for his protection. more So far, he passed through the Tricastini: thence he takes the valley of the Drac e Mt. through the Vocontii, who lived about the Vizille, and came into the country of the le pre-Tricorii, about Corps and St. Bonnet. e, as I St. Bonnet he has to cross a pass 4,000 feet above the sea, to Gap (the ancient Vapincum). as reorough It is true that he might have saved fifty t with miles in distance by leaving the Drac at s con-Vizille and striking due east for the Genevre; but that involves crossing the main Col de Lauteret, a pass nearly 7,000 feet m the high, 600 feet higher than the Genèvre , and, itself, and very much colder and more wintry, as being close to the great glaciers of upon the Dauphine Alps. Pompey afterwards h conopened this route, to save time on the road d have south to Gaul and Spain; but it would have been he Po. surprising if Hannibal's guides had taken s subhim there, instead of by the easier route tion of which the Romans used until Pompey's time, seems and which Livy fixes for us by his mention

of the Tricorii and the river Druentia.

From St. Bonnet, then, he crosses the 'Col

de Bayard '-the name, a record of Francis

L's passage, when Bayard, the last of the

aurini,

e that

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knights of chivalry, was marching to his death on the banks of the Sesia-and this Col Bayard I should follow Mr. Freshfield in making the ἀναβολή, or first ascent.1 Here the natives guarded the pass, as both Polybius and Livy tell us, by day, and departed at night to their town beyond; and, though Hannibal took advantage of their rather crude outpost system to crown the ridge by night, yet he had considerable difficulty and danger in getting through the pass next day, and occupying the townpossibly where Gap now stands. Here he entered the valley of the Druentia near Chorges (which preserves the name of the Caturiges). Difficulty may be felt-indeed, it is made an insuperable difficulty by some -in the settlement of Allobroges, about Gap, so far south of the Isère: but I am inclined to think that Polybius does not place the Allobroges in the island: He mentions 'barbarians' who dwelt on the island, but gives them no name, and he clearly distinguishes the 'barbarians' of the island who guided Hannibal from the 'Allobroges' who attacked him near the ἀναβολή after the escort had left him. I suspect that in 218 B.C. the Allobroges lived to the south of the island, and that before Livy's time they had migrated northwards and occupied the island.

The crossing of the Druentia must, if we follow Polybius's days, be placed after the engagement at the ἀναβολή—not before, as Livy says; otherwise we are brought too near to the summit of the pass. But this is a divergence of quite a different kind from that which we should admit, if we said that Hannibal never touched the Druentia at all. Wherever it came on the march, the passage of this river was a difficulty; and that difficulty Livy had heard of and describes, mentioning both the name of the river and its natural features. I agree with Mr. Freshfield in regarding this as the only difference worth mentioning in the narratives of Livy and Polybius, unless we must add as a discrepancy the position of the Allobroges, of which I spoke above.

Up to this point the route is the same for the Argentière and Genèvre. Here the route to the Genèvre turns along the Dur-

¹ I feel sure that Mr. Freshfield is right in taking this ascent to be the ἀναβολή of Polybius iii. 50. He tells us that, after marching 10 days 'ἐν τοῖς ἐπιπέοις' he 'ἡρξατο τῆς πρὸς τὰς 'Αλπεις ἀναβολῆς,' evidently his first really steep ascent. Dr. Fuchs takes this to be at Embrun in the valley of the Durance: but is it likely that crossing a watershed 4,000 feet above the sea would be unnoticed, and the slighter ascent in the river valley be made the first ἀναβολή?

ance Valley, north-east to Briançon, about thirty-five English miles. Briancon is seven and half miles by the present coach road from this summit of the pass, and there is a rise in that distance of 1,700 feet. We know that Hannibal came to a rocky gorge within a day's march of the summit, where the Gauls, who had feigned friendship, attacked him: that, with some difficulty, he passed his cavalry and baggage through, while he himself, with the infantry, guarded the rear, and that he halted his infantry for the night near a white rock. Next morning he joined the cavalry, and the same day reached the summit. It is probable that this halt was somewhere about Briançon. Any one who cares to localise it may fix upon the rock on which the fortress of Briançon stands, as the veritable λευκόπετρον όχυρόν.

The summit of the pass of Genèvre is a plateau nearly two miles across, giving ample space for encampments. No reasonable man will trouble himself about the view from the top. In fact, of all the competing passes, the Cenis is the only one from the top of which any Italian view can be seen. The little St. Bernard is the worst of all in this respect, since it gives merely a magnificent view of Mt. Blanc, which the weary Africans would not find cheering. But in truth, on whatever pass he stood, we need not suppose more than that he pointed to the descent, and in the direction of the plains which he promised them. Among other things Polybius says that Hannibal pointed out Rome itself, which no one but the Governor of Tilbury Fort could suppose him to be describing as actually in sight.

From the summit to Susa is a distance of twenty-seven miles, which could easily be traversed within three days. It is possible, however, that the point where he is regarded as having reached the plains, and where he halted, is Avigliana, where the Susa valley widens out. This is eighteen miles further on, and would make his three days' march forty-five miles, not an impossible distance.

Hennebert, who is correct in taking Hannibal over the Genèvre, seems to me to go quite wrong at this point. He lays great stress upon the words in Strabo iv. p. 203. "Δουρίας κατενεχθείς διὰ Σαλάσσων, as showing that the Salassi lived also in the valley of the Dora Riparia, which, he contends, would make it impossible to believe that Hannibal went through it. He therefore claims to have proved (p. 90) that the route after the descent from the Genèvre was through the Val Chisone — though this forces Hannibal, after reaching the valley at

Césanne, to climb out of it by a second pass, the Col de Sestrières, a height of 6631 feet, nearly 600 feet higher than the Genevre itself! I think that the answer to his difficulty is this: it may be gathered by comparing page 205 that Strabo confuses the other Dovpias, the Dora Baltea, which flows through the valley of Aosta, in which the Salassi dwelt, with the Dora Riparia, which flows from the Genèvre past Susa and Avigliana, and this confusion has made him speak of Salassi as dwelling, apparently, about the Dora Riparia. But more than this, even if there really was a branch of the Salassi in that valley also in Strabo's time, it does not follow that they had spread there two centuries earlier. The failure to understand that Celtic tribes shifted their territorial limits has been a fruitful source of error. A known position of a tribe at a later time may be used to support a conclusion by probabilities, but is certainly not conclusive in itself.

If Hannibal's halt was at Avigliana, this would make his total march from St. Bonnet 128 English miles, which (though for reasons mentioned above, it should not be used as an argument) happens to be very near the estimate in Polybius, 150 M.P. (= 137 English miles), from the $dv\alpha\betao\lambda\dot{\eta}$ to the plains, and the total distance we have brought him is within six miles of the 250 miles derived from Polybius.

The descent on the Italian side of the Genèvre is by a valley with steep sides, which would suit the description well enough, neither better nor worse than any of the competing passes. Mr. Freshfield is inclined to postulate 'a gorge with a hill track turning it,' which he finds in the 'Gorge of Barricades' in the Argentière. I do not think that he interprets the words of Polybius exactly as I understand them. The account, as I read it, in Polybius and Livy is this. Hannibal, having halted two nights on the top, began his descent on the second morning. After going a little way he found that the path, which wound down one side of the valley had been broken away by a recent landslip for a distance of a stade and a half, or nearly 1000 feet-a difficulty which effectually stopped him. And here I should like for a moment to remark upon the unfair treatment meted out to Livy. He writes, 'this place naturally steep, now recenti terrae lapsu impeditus in mirandam admodum altitudinem abruptus erat.' Literally, 'was

¹ Dr. Fuchs (p. 128) does not appear to observe that the text which he cites is merely a suggested

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nent to meted naturlapsu altitu-, 'was observe suggested

by a recent landslip made impassable, being broken away to a surprising depth.' How deep he does not say, but a drop of even 100 feet would be startling. So the MSS; but not so the editors who alter, following what they are pleased to call 'the almost certain emendation of Valla,' to 'in pedum mille admodum altitudinem,' a depth of about 1000 feet, and then they abuse Livy.1 'Polybius,' says a very excellent scholar, 'writes three half stades in distance. Livy mistakes this, or the language of their common authority, and converts it into a precipice of 1000 feet in depth, if we accept Valla's almost certain emendation.' 'This,' says Niebuhr, 'is nonsense, as all must see.' Of course, it would be an exaggeration, though those who are familiar with alpine tracks would think 'nonsense' too strong a word. But why substitute it for Livy's sense? Such perversity makes one glad that the days when an 'almost certain emendation' was an almost certain step to a bishopric, have passed away. But if 'certain emendations' can no longer affect the Church, they may still sometimes vitiate the sources of history.

Hannibal, thus suddenly brought to a standstill, tried to make a circuit by getting along the face either above or below the landslip, probably above. This he found too dangerous, as it involved passing across a slope where fresh snow rested in imperfect cohesion on old hard snow. So he went up to the ράχις or ridge. Livy calls it jugumthe exact French translation would be arête, -and encamped there, probably not on the summit of the pass, but simply on the nearest part of the edge which forms one side of the valley, as he could not halt his army on the slope. I see nothing about a 'narrow gorge with a possibility of turning it by a side path,' and I think that Mr. Freshfield in so explaining it has misunderstood the statement of Polybius, that they could not get by the bad place διὰ τὴν στένοτητα. This does not mean the narrowness of a gorge, but the narrowness of the path, which had nearly all been swept away,2 and Livy's

emendation. I cannot follow him either in his argument that the passage so emended 'in pedum mille altitudinem' has the same meaning as the words of Polybius 'επὶ τρι' ἡμιστάδια τῆς ἀπορρώγος οὐσης.' Folybius 'êm' τρι' ἡμιστάδια τῆς ἀπορρώγος οδοης. The Greek can only mean horizontal distance (or distance measured along the path), and in altibutinem can refer to nothing but vertical depth.

1 The sentence would no doubt run more smoothly if we had 'et' after impeditus, or placed 'erat' there; but even without change it may stand as the MSS. have it

This is rightly taken by Dr. Fuchs, p. 99.

expression 'angustior rupes' is probably meant to convey the same idea. It took a day's work to repair the paths sufficiently to get by, zigzagging it in some parts, Livy says, and then the army marched on the resting place in the valley. It may be well to say a few words about two points in the details of the descent. First, the famous vinegar story. Livy probably had an exaggerated idea of the extent of the zigzags actually cut in the rock, but I believe that much of the ridicule which has been directed against him is caused by an idea that he, as well as Juvenal, said 'montem rumpit aceto.' What Livy does say is that the soldiers, having to repair the paths down the rocky slope, or to remake it, cut down trees and made a large fire on the rock: the heat and the vinegar poured on it made the rock crumbly, so that they were able to cut it more easily. The 'huge trees' are probably an exaggeration; but wood of some kind might well be granted. As to the use of fire, among other instances the farmers of Salisbury Plain, when they wanted barbarously to break up the great blocks about Stonehenge, used to light a fire of sticks upon the stone and, when it was hot, pour water on it to make it crack. Further, as regards the use of vinegar, we have Pliny's testimony that it was used to aid in disintegrating rocks, and that fire was used too.3

The other question is what was the old snow of which both Livy and Polybius speak? We are told that over the old snow was fresh snow, 'modicae altitudinis,' in such a state that it slipped away with those who trod on it, and left the base ice. Every one who knows the Alps is familiar with the danger caused by fresh snow falling on a frozen slope. On steep ground the two layers take quite three days to cohere, and the upper layer will probably break away in an avalanche if it is trodden on soon after it falls. There may have been a fall of snow which had hardened some days earlier and been coated by a recent snowfall. But if Polybius is correct in calling the old layer a remnant of snow from the winter before,4 it must have been an avalanche bed which was thus coated, for there is not the

³ Plin. xxiii. §§ 54, 57 'Aceto summa vis est in refrigerando, non tamen minor in discutiendo...saxa rumpit infusum, quae non ruperit ignis antecedens.' Cf. Plin. xxxiii., ch. 21. Hennebert, ii. 260, cites Dio Cassius, and Apollodorus, the architect, to the

same effect.

4 ' ἐπὶ γὰρ τὴν προϋπάρχουσαν χιόνα καὶ διαμεμενη-κυΐαν ἐκ τοῦ προτέρου χειμῶνος ἄρτι τῆς ἐπ' ἔτου: πεπτωκυίας ' (iii. 55).

smallest ground for supposing that 2000 years ago the line of perpetual snow was at a greatly lower level than it is now, There is nothing impossible in avalanche beds, thick and sheltered by a gully, lasting through the summer. They are often met with as low down. But whether we take this first snow to have been lying on hardened snow which had fallen a few days earlier, or on an old avalanche bed, the description of what followed bears all the stamp of truth, and suggests accurate information.

The argument upon which the St. Bernardites lay the greatest stress is that, according to Polybius, Hannibal should descend upon the Insubres or "Iv $\sigma o\mu \beta \rho o\iota$, whereas the southern passes bring him to

the Taurini.

Their own route to the Val d'Aosta, as I pointed out, brings him to the Salassi: but let that pass. Does Polybius exclude a descent into the country of the Taurini, and so contradict Livy, who says 'The Taurini were the nearest people to him when he came into Italy'? Polybius, speaking generally of the march, says (iii. 56) 'he descended with confidence (τολμηρῶς) into the plains about the Po and the tribe of the Insubres.' I take this to mean simply that the Insubres, a friendly people, were the dominant tribe in Cisalpine Gaul about the Po, and therefore he went confidently into that districtwith more confidence, for instance, than he would have gone by the coast road into the That he passed the evi-Ligurian country. dently weak Taurini, and stormed their town in passing to reach the Insubres, does not contradict the statement that the Insubres were to be his stand-by in the plains of the Po. When we come to Polybius's detailed account of the passage and Hannibal's subsequent movements, we find no indication of an immediate meeting with the Insubrians. On the contrary the history (Pol. iii. 60) tells us that the army, being reduced to the state, so to speak, of brute beasts (οἶον ἀποτεθηριωμένοι) by the mountains, Hannibal encamped to rest his troops under the very skirts of the Alps (ὑπ' αὐτὴν τὴν παρωρείαν τῶν 'Αλπέων) and that then (μετὰ ταῦτα), since the Taurini, who happen to dwell near this spot (πρὸς τῆ παρωρεία κατοικοῦντες), were at enmity with the Insubrians and did not listen to his overtures, he stormed their chief town (Turin, or thereabouts). There is not a word of cooperating with the Insubrians against them, as we should expect if he had descended into the Insubrian country at Ivrea, and then turned back

westward to storm Turin. I confess that, if I had no authority but Polybius, I should still conclude from him that the Taurini were the first tribe that Hannibal came across in Italy.

It only remains to see if the brief notices in Varro and Strabo agree with the view

here advocated.

Strabo says that Polybius mentions four passes as used by the Romans: (1) that through the Ligurians, (2) then that through the Taurini which Hannibal crossed, (3) then that through the Salassi, (4) then

through the Raeti.

(1) is the Cornice Road, (2) must be the Argentière, the Genèvre, or the Cenis, Polybius only knowing one of those three passes. (3) is the little St. Bernard, (4) the Septimer from Coire to Como. As to (2) which this passage assigns to Hannibal, Polybius was more likely to have ignored the Argentière or Cenis than the Genèvre; for we have stronger evidence of an early use of the Genèvre by the Romans than can be found for either of the other two.

Varro gives five passes as possible for crossing the Alps, (1) the Ligurian, (2) that by which Hannibal passed, (3) that by which Pompey went to the Spanish war, (4) that by which Hasdrubal entered Italy, (5) the Alpes Graiae. The Graian pass is the Little St. Bernard: therefore Hasdrubal's is, according to Varro, the Cenis. The difficulty ensues that, if we assign the Genèvre to Hannibal, we may seem to have no pass for Pompey, and this is naturally made an argument for making the Argentière Hannibal's pass. I have argued for the rejection of the Argentière from the narratives of Polybius and Livy, because it brings Hannibal too far to the south; and I think it probable that that pass (which is not in the Itineraries) was unknown to the Romans in Varro's time. The explanation which I should offer is, that Pompey, pressing towards Spain, opened the more direct route from the Genèvre across the Col de Lauteret, and by the valley of the Romanche. This was, as we know, afterwards used as the direct Roman road to Gaul, 54 miles shorter than the way by Gap; and, as its highest point in the Col de Lauteret involves another and a higher ascent, and lies far to the north of the older route by the Durance valley and Gap, Varro gives it as a distinct pass from Hannibal's, although the road on the Italian side as far as the summit of the Genèvre, and a few miles on the French side is the same. It is in fact hardly less allowable to talk of them

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as distinct passes, than to distinguish the ss that. Septimer and Julier passes which coincide bius, I for 4 of their length. I have a strong hat the opinion that the words of Pompey's letter, annibal quoted in a fragment of Sallust, (Hist. iii. 3) Per eas [Alpes] iter aliud atque Hannibal, nobis opportunius patefeci," gives support to this view. They may well be intended to notices he view point out that this new and shorter variation, ns four by the Lauteret route, branching off just be-1) that low the Col de Genèvre, and then climbing the through much higher watershed, was, because of its

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The conclusion, then, to which I am

directness, a more convenient military road,

brought is that the narrative of Polybius suits best the Genèvre, which (or the Argentière) Livy distinctly requires us to adopt; and that Varro's account probably, and the citation in Strabo certainly, support the same view. All four accounts are absolutely impossible to reconcile with the Little St. Bernard. The Col de l'Argentière, which Mr. Freshfield prefers, is a possible, though as it seems to me, a less probable alternative. I agree with Mr. Freshfield in holding that, besides these two neighbouring passes, there is no other which can be accepted without throwing over either Polybius or Livy, or both at once.

G. E. MARINDIN.

NOTES ON GREEK COMIC FRAGMENTS.

(Continued from p. 150).

ALONG with the fragments of Menander is usually printed a large collection of Γνώμαι μονόστιχοι, or apophthegms contained usually in a single line, traditionally derived from his plays. Some of them come from him, some from other comic poets, many from tragedy, others from other sources, and many no doubt are late. The MS. evidence for them is at present so uncertain (see Kock's Preface to his 3rd volume and also his article in the Rheinisches Museum, Vol. 41) that it might be the part of wisdom to follow Kock's example and not deal with them at all, the more so since their absence from his edition makes it difficult to ascertain what scattered conjectures have been proposed for the improvement of the text. I will therefore omit some things that suggest themselves and state briefly s few that appear to be best worth putting down. I follow the text and numbering of Meineke's larger edition of the Comic Fragments.

30. 'Ανήρ ἄριστος οὐκ αν είη δυσγενής.

A superlative is quite out of place. Perhaps ἀνηρ ὁ χρηστός. Cf. Meineke's probable conjecture of χρηστά for ἄριστα in Aesch. Sept. 183 ἢ ταῦτ ἄριστα καὶ πόλει σωτήρια κ.τ.λ. and Orelli's ἄριστον for ἄχρηστον in Aen. Tact. 2, 1.

65, 66. Βιοῖ μὲν οὐδεὶς ὃν προαιρεῖται βίον. Βίος κέκληται δ' ὃς βία πορίζεται.

These two lines should be joined together and ore read for os. It is not clear from

Meineke's note whether there is any MS. authority for ὅτι.

109. γυνη γυναικός πώποτ' οὐδὲν διαφέρει.

Πώποτ' is indeed a miserable makeshift, as Kock says. It cannot precede the negative nor go with a present tense. Perhaps γυναικὸς οὐδὲν διαφέρει γυνή ποτε (or πάνυ), or γυνή γυναικὸς διαφέρει γὰρ οὐδὲ ἔν.

119. Δίκαιος ἴσθι ἴνα δικαίων τύχης.

Non est antiqui poetae says Meineke, but δίκαιος ἴσθι τῶν δικαίων ἴνα (ὡς ?) τύχης might be.

158. Έαυτὸν οὐδεὶς ὁμολογεῖ κακοῦργος ων.

The meaning should be 'no one admits to himself' $(\pi\rho \delta s \ a\delta \tau \delta v ? \ a\delta \tau \hat{\varphi} \ \gamma \delta \rho ?)$: a statement not more untrue than Juvenal's se iudice nemo nocens absolvitur.

167. Εὐκαταφρόνητός ἐστι σιγηρὸς τρόπος (σιγηλός Blaydes).

It will be another example of a well-attested error, if this stands for ἀκαταφρόνητος (Hirschig οὐ καταφρόνητος). The point may resemble that of Eur. Med. 319-20, or that of Ar. Ran. 916.

203, 204. "Ηθη πονηρὰ τὴν φύσιν διαστρέφει.
"Ηθος πονηρὸν φεῦγε καὶ κέρδος κακόν.

"H $\theta\eta$ and $\eta\theta$ os should be $\tilde{\epsilon}\theta\eta$ and $\tilde{\epsilon}\theta$ os.

206, 207. "Ηδιστόν έστι τῶν ὑπαρχόντων κρατεῖν.

"Ηδιστόν έστιν εύτυχοῦντα νοῦν ἔχειν.

'Pleasantest' is evidently wrong. 'Best' (ἄριστον) or 'difficult' or 'uncommon' would make sense, but I do not see clearly what the Greek word is likely to have been. Was it ἐθιστόν or ἐθιστέον ?

221. Ἡδὺ σιωπᾶν ἡ λαλεῖν ἃ μὴ πρέπει.

Here again ἡδύ must be wrong for two reasons, and this time it is easy to see what the original word probably was. Read ἐλοῦ (ΕΛΟΥ for ΗΔΥ), for ἐλοῦ ἡ comparing Dem. Ol. 2, 22 ἐλοίμην ἡ, and other passages.

229. Θεὸν σέβου καὶ πάντα πράξεις ἐνθέως.

For $\epsilon\nu\theta\epsilon\omega_S$ we should read $\sigma\dot{\nu}\nu$ $\theta\epsilon\sigma\hat{\imath}s$ 'with heaven's help.' ' $E\nu$ and $\sigma\dot{\nu}\nu$ are often confused.

235. Θησαυρός έστι τοῦ βίου τὰ πράγματα.

I conjecture γράμματα for πράγματα, as the words are sometimes confused. See Schäfer's *Greg. Cor.* 1, 185 note and elsewhere. For the sense compare such other γνώμαι as

- 312. λιμήν πέφυκε πᾶσι παιδεία βροτοῖς.
- 403. δ γράμματ' είδως και περιττον νουν έχει.
- 438. ὁ γραμμάτων ἄπειρος οὐ βλέπει βλέπων.
- 652. βακτηρία γάρ ἐστι παιδεία βίου.
- 657. διπλοῦν ὁρῶσιν οἱ μαθόντες γράμματα.

with the saying which Diogenes (5, 1, 21) attributes to Aristotle: $\kappa \dot{\alpha} \lambda \lambda \iota \sigma \tau o \nu \dot{\epsilon} \dot{\phi} \dot{\phi} \dot{\phi} \iota v \tau \dot{\phi}$ $\gamma \dot{\eta} \rho a \tau \dot{\eta} \nu \pi a \iota \delta \dot{\epsilon} \dot{\alpha} \nu \dot{\epsilon} \lambda \epsilon \gamma \epsilon$, and Diodorus 12, 13, 2 (end).

This seems much more likely than Kock's attempt (Rhein. Mus. 41, 90) to connect this line in meaning with Aesch. Fab. 22: ὁ μῦθος δηλοῖ ὅτι ὁ κάματος θησαυρός ἐστι τοῖς ἀνθρώποις.

257. ἴσος ἴσθι πᾶσι κᾶν ὑπερβάλλης βίφ. Read βία. Cf. Soph. Aj. 130.

318. Λιμὴν πλοίου μέν, ἀλυπία δ' ὅρμος βίου.
Read λιμὴν πλοίου μὲν ὅρμος, τοῦ βίου δ' ἀλυπία.

Cf. Meineke's note.

328. Λάλει μέτρια καὶ μὴ λάλει ἃ μή σε δεῖ.

Perhaps μέτρια λαλήσεις μὴ λαλῶν ἃ μή σε δεῖ.

337. Μισθὸς διδάσκει γράμματ', οὐ διδάσκαλος.

Μισθός seems unmeaning. Perhaps μ όχθος, as in the next line: μ οχθεῖν ἀνάγκη τοὺς θέλοντας εὐτυχεῖν.

362. μή γάμει γυναίκα κούκ άνοίξεις τάφον.

I would either read $\mu\dot{\gamma}$ | $\gamma\dot{a}\mu\epsilon\iota$ $\gamma \nu \nu a i \kappa a \kappa o i \kappa$ $\dot{a}\nu o i \xi \epsilon\iota s < \sigma o \iota > \tau \dot{a}\phi o \nu$, or still inserting $\sigma o \iota$ regard the line as part of a trochaic tetrameter. There are many such commingled with the iambics in the Sententiae of Publilius Syrus, and some of these $\gamma \nu \hat{o}\mu a \iota$ may very well have had that form originally. So 318 above.

367. Μετά δικαίου ἀεὶ διατριβάς ποίει.

Rather μετά των δικαίων διατριβάς άεὶ ποιού.

376. Νικά γὰρ ἀεὶ διαβολή τὰ κρείττονα.

Should we alter νικα to some verb meaning 'aims at' or 'assails'? Possibly δάκνει.

385. Νὺξ μὲν ἀναπαύει, ἡμέρα δ' ἔργον ποιεῖ. Read ἀνάπαυσιν. Is ποιεῖ right?

424. οὐδεὶς ὁ νοεῖς μὲν οἶδεν, ὁ δὲ ποιεῖς βλέπει.

This could only mean 'no one knows what you think and sees what you do.' Read δ νοάς μὲν οὐδεὶς σίδεν, ὁ δὲ ποιεῖς βλέπει, i.e. 'what you think, no one knows, but (he) sees what you do.' The change from οὐδείς and nemo to a positive idea is common enough: e.g. Plat. Symp. 192 Ε οὕδ' ἄν εῖς ἐξαρνηθείη... ἀλλά...οἴοιτ' ἄν κ.τ.λ.: Hor. S. 1. 1. 1-3. For the meaning cf. Publilius Syrus Sent. Append. 11 (Ribbeck Com. R. Fragm.) Cernuntur facta, nemini animus cernitur.

439. οὐδεὶς πῦρ εἰς χρήματα διδοὺς ἐπαύσατο.

Meineke says in πῦρ εἰς latet genitivus substantivi dolorem vel malum indicantis, and πυρός or πυρετοῦ has been conjectured. Rather, I should say, the words conceal an aorist passive participle in -εις, and the well-known ὁ μὴ δαρεὶς ἄνθρωπος οὖ παιδεύεπαι (422) suggests that δαρείς is the word. This very day (Nov. 6, 1896) it is reported in the Times that Mr. Justice Wills thought 'a punishment so irrevocable as flogging should not be added to the existing punishment.'

446. πολλούς ὁ καιρὸς οὐκ ὅντας ποιεῖ φίλους.

Perhaps πολλοὺς ὁ καιρός, οὐχ ὁ νοῦς, ποιεῦ φίλους, circumstances (or interest), not real feeling. Transposition of the words is easy enough, but emendation gives a much more pointed sense.

523. "Υπνος δεινον ανθρώποις κακόν.

Υπόνοια has been suggested, but I fancy the original line was ὖπνος περιττὸς δεινὸν ἀνθρώποις κακόν. Cf. 88 γέλως ἄκαιρος ἐν βροτοῖς δεινὸν κακόν. Cf. Plato, Laws 808 B: Diog. I. 3. 39 against too much sleep. (I find from Kock in R.M. that Meineke in his editio minor proposed ὅπνος δ΄ ἄκαιρος). So Webster in the Duchess of Malfi, 1. 1.

If too immoderate sleep be truly said To be an inward rust unto the soul.

590. Φίλος φίλου δεόμενος οὐκ ἔστιν φίλος.

Unmeaning. The author wished perhaps for a candid friend and wrote φίλος φίλον φαδόμενος οὐκ ἔστιν φίλος. Cf. Syrus Sent. 10, amici vitia si feras, facias tua.

606. Ίσχυε σοφία κάρετη, χρόνω δὲ μή.

For $\chi\rho\delta\nu_{\psi}$ we might read $\lambda\delta\gamma\psi$ ($\chi\rho\delta\nu_{0}$ s et $\lambda\delta\gamma_{0}$ s ob similitudinem compendiorum quibus scribuntur, χ (frequentior est nota inversa χ et χ assim confunduntur Bast in Schäfer's Greg. Cor. 1. 33, note), meaning mere power of words.

636. Ψυχής άρχαίας οὐδέν ἐστι γλυκύτερον.

'Ακεραίας Meineke: $i\lambda a\rho \hat{a}\varsigma$ Schmidt. Perhaps δικαίας. The three letters a ρ χ are known to get confused respectively with δ ι κ . But are just people always the pleasantest?

644. 'Ανάπαυσίς ἐστι τῶν κακῶν ἀπραξία.

This might be taken to mean that a man can escape troubles by doing nothing at all, and some one may compare Syrus Sent. 377, nil agere semper infelicist optimum. That, however, means rather that a man had better not try his luck, if it is habitually bad: and here ἀνάπανοις must mean cessation, not avoidance. When in trouble, a man cannot get out of it by mere ἀπραξία. Was the real word ἀπαραξία, meaning that your troubles cease to exist if you cease to concern yourself with them? Not to feel them is not to have them.

653. Βραβείον άρετης έστιν εὐπαιδευσία.

Excellence does not, however, bestow a good education. It is good education that bestows excellence. Perhaps therefore we should read something like βραβεῖον ἀρετή 'στιν εὐπαιδευσίας.

682. λεπτῶς γέ τοι ζην κρείσσον ἡ λαμπρῶς

For $\lambda \epsilon \pi \tau \hat{\omega}_s$ $\gamma \epsilon \tau \omega$ Blaydes writes $\lambda \iota \tau \hat{\omega}_s$ $\kappa a \lambda \hat{\omega}_s$, Schmidt $\lambda \epsilon \pi \tau \hat{\omega}_s$ $\gamma \hat{\alpha} \rho \epsilon \hat{v}$. $\Lambda a \mu \pi \rho \hat{\omega}_s$ $\kappa a \kappa \hat{\omega}_s$ also seems to me doubtful, and I suggest $\lambda \iota \tau \sigma \hat{\upsilon}_s$ $\gamma \hat{\alpha} \rho \epsilon \hat{v}$ $\xi \hat{\eta} \nu \kappa \rho \epsilon \hat{\iota} \tau \tau \sigma \nu \hat{\eta}$ $\lambda \alpha \mu \pi \rho \sigma \hat{\upsilon}_s$ $\kappa a \kappa \hat{\omega}_s$, the adjectives going closely with the verb

 'Ρέγχει παρούσης της τύχης τὰ πράγματα.

Is $\dot{\rho}\dot{\epsilon}\gamma\chi\epsilon\iota$ anything but a blunder for $\tau\rho\dot{\epsilon}\chi\epsilon\iota$ 'things go on swimmingly'? Cf. Pind Pyth. 8. 32, $\tau\dot{o}$ δ' $\dot{\epsilon}\nu$ $\tau\sigma\sigma\dot{\iota}$ μοι $\tau\rho\dot{\alpha}\chi\sigma\nu$ $\dot{\iota}\tau\omega$, where, however, $\tau\rho\dot{\alpha}\chi\sigma\nu$ is not always taken with $\dot{\iota}\tau\omega$.

HERBERT RICHARDS.

CONTRACTED FORMS OF THE PERFECT IN QUINTILIAN.

The fact that Quintilian himself in two different passages has expressed his own views in regard to the use of the longer or shorter form of the perfect, lends an additional interest to the observation of his own practice in this direction. In ix. 4,59 he sets 'ratio' in the form vitavisse over against 'consuetudo' in the form vitasse, leaving the final choice to be determined in each case by 'compositio.' In i. 6, 17-21, in a more extended discussion of these and

similar forms, he has expressed himself somewhat more strongly. Here he makes the remark, that it is only those 'with a most unpleasantly perverse attachment to exactness' who would use audivisse and scivisse, and concludes the passage by saying, 'let the extremely learned man say conservavisse rather than what we say' (conservasse). This gives no uncertain hint as to his own preference. For the usage of the time of Cicero we have that writer's own words, to

εὶ ποιοῦ. ονα.

EL.

ου διδάσ-

aps µóx-

έγκη τούς

τάφον.

аска койк

ting out

ic tetra-

mingled

f Publi-

uau may

lly. So

neaning ενει.

ον ποιεί.

ποιείς

vs what d \hat{o} voe \hat{i} s what es what d nemo h: e.g. $\eta\theta\epsilon(\hat{\eta}...$ 1. 1-3. s Sent.

Fragm.)
tur.

zύσατο.
nitivus
icantis,

ctured.
eal an
id the
δεύεται
word.
ported

ποιεί

ogging

unish

be found in Orat. § 157, to the effect that in his time the contracted forms were usual, but that the fuller form 'recte dici et imminutum usitate.' For this period we have also a statement of Varro, L. L. iii. fr. p. 148 (W.), that amasti, nosti, abiit are the favourite forms of the time. For the usage of a still earlier period, cf. Lindsay, Lat. Lang., p. 507, chapt. viii., § 48, who says that Terence usually adopts the contracted forms in -evi, -ivi and of novi, though the forms in avi are usually contracted only at the end of the line; and that in Plautus the uncontracted form, though used in the middle of the line, is not used at the end. ' Eo,' he says, 'and its compounds have even in Plautus usually the form ii in perfect forms, except ivi, exivi, ambivi, etc.' the further usage of poets cf. Luc. Mueller, Re Metr.² p. 507 and 508, and for Claudian, Birt, Archiv. f. lat. Lexicogr. iv., p. 589; Reisig, Vorles. Neub. von Hagen i., p. 376, Note 271 says: 'Über Quint., S. Bonnell lexic. Quint. prolegg. p. xxvii.; bei ihm ist besonders die contraction nach a haüfig, namentlich in conj. perf.; von e und o führt Bonnell nichts an; jedoch ist wenigstens nosse nicht selten, andere Formen aber mochten sich selten finden.' Neue-Wagener, Formenlehre,3 p. 430 fol. gives an exhaustive list of the contracted perfect forms, but his list for Quintilian is far from complete. On p. 476 he cites four occurrences of obisse, which should be perisse; on p. 488 he cites noritis 1, 1, 25 for norint.

The object of the present investigation is to give a complete list, classified, of all contracted perfect forms in Quintilian, and to determine more exactly the usage of this

writer

I. Infinitive Forms.—Quintilian never uses the fuller form of the infinitive. The verbs most frequently contracted are those of the first conjugation. Of these forty

different verbs occur.

(a) First conjugation: adnotasse 8, 6, 2; cogitasse 9, 2, 30; commendasse 5, 13, 30; concitasse 11, 3, 8; cubasse 7, 2, 20; curasse 1, 1, 34; damnasse 9, 3, 90; demonstrasse 2, 6, 2; donasse 5, 10, 112; dubitasse 9, 2, 20; emendasse 11, 3, 130; errasse 3, 4, 4: 8, 6, 2; exanimasse 12, 10, 4; exarasse 9, 4, 90; exclamasse 1, 6, 45; expectasse 9, 2, 39; factitasse 7, 2, 26: 10, 5, 2: 12, 3, 4; habitasse 8, 6, 71; ignorasse 9, 1, 18; inclinasse 10, 1, 80; inlustrasse 3, 7, 10; iudicasse 5, 9, 13: 11, 3, 6; lacerasse 8, 2, 20; narrasse 4, 2, 10: 10, 3, 12; nominasse 4, 1, 1; numerasse 5, 7, 5; obscurasse 5, 13,

41; occupasse 2, 21, 13; percasse 7, 4, 31; pererrasse 5, 11, 13; perseverasse 11, 1, 80; postulasse 11, 1, 58; salutasse 8, 4, 2; servasse 3, 8, 51; signasse 5, 11, 32; separasse 12, 1, 2; superasse 10, 2, 28; terminasse 8, 4, 40; tractasse 3, 1, 12: 5, 13: 5, 10, 37; and vitasse 3, 3, 5 (forty verbs).

(b) Third conjugation: here only nosse is used, occurring ten times, 4, 1, 17: 5, 23: 5 proem. 4: 5, 7, 26: 6, 4, 8: 7, 1, 4: 10, 1, 22: 5, 20: 12, 2, 29: 4, 2 (one verb).

(c) Fourth conjugation is somewhat more common; audisse 3, 1, 17: 12, 8, 8; coisse 5, 9, 5: 11, 35; isse 7, 1, 30; perisse 5, 9, 11: 7, 2, 23: 10, 7, 4: 11, 2, 15; redisse 7, 1, 30; servisse 7, 2, 26, and scisse 7, 4, 14; 12, 11, 24 (seven verbs).

II. Indicative Forms.

(a) Perfect, 2nd pers., rare: efflagitasti Praef. I; existi 5, 10, 45; militasti 7, 1, 51; stuprasti 4, 2, 71; vulnerasti 6, 2, 23, and only once in the plur., damnastis 5, 10, 79 (six verbs).

(b) Perfect, 3rd plur., more common.

(1) First conjugation: depravarunt 9, 3, 100; donarunt 8, 5, 12; errarunt 1, 1, 32; existimarunt 10, 15, 10: 3, 11, 20: 5, 12, 21: 6, 67; impugnarunt 2, 17, 40; indicarunt 4, 2, 72; intrarunt 4, 2, 72; laborarunt 3, 8, 52: 11, 22: 9, 2, 77; negarunt 7, 4, 17; notarunt 3, 2, 3; nominarunt 9, 2, 23; pararunt 8 proem. 29; pronuntiarunt 1, 5, 60; signarunt 4, 1, 3; vindicarunt 2, 17, 40 (fifteen verbs).

(2) Third conjugation, only two examples: desirrunt 5, 10, 101; norunt 1, 9, 6.

(3) Fourth conjugation, only three examples: audierunt 4, 2, 38; coierunt 11, 3 103; scierunt 9, 4, 4.

(c) Pluperfect only in 3rd pers. sing

except coniurarant 4, 2, 72.

(1) First conjugation, only comparat 10 1, 79.

(2) Third conjugation, only norat 12, 10

57 and petierat 4, 2, 85; 6, 3, 68.
(3) Fourth conjugation, only three examples: audierat 12, 6, 7; exierat 11, 2, 11; redierat 9, 3, 73.

(d) Future perfect.

(1) First conjugation: in the 2nd pers. sing. only indicaris 9, 3, 68; and 1st pers. plur. only accomodarimus 6, 2, 26 and existimarimus 10, 3, 29; more common in the 3rd sing.: compararit 3, 2, 2; invitarit, 7, 3, 3; optarit 12, 7, 4; segregarit 1, 2, 21; tractarit 12, 2, 3.

(2) Fourth conjugation: audierit 10, 5, 20: 11, 2, 34; exierit 4, 2, 61; perierit 7, 3, 33; scierit 5, 13, 52: 8, 6, 16: 9, 2, 4;

transierit 10, 2, 10: 12, 6, 7 (five verbs); in the 1st pers. plur. only scierimus 5, 10, 22 and 10, 7, 5.

III. Subjunctive Forms.

(a) Perfect.

7, 4, 31; 1, 1, 80;

, 2; ser-

separasse asse 8, 4,

10, 37;

nosse is

: 5, 23:

4: 10,

at more

; coiisse

sse 5, 9,

edisse 7,

, 4, 14:

agitasti

7, 1, 51;

23, and

10, 79

nt 9, 3,

1, 32; : 5, 12,

icarunt

runt 3,

4, 17; 2, 23;

it 1, 5,

2, 17,

mples:

ee ex-

11, 3

. sing

12, 10

ee ex-

2, 11;

l pers.

d 1st

26 and non in vitarit, 2, 21; 10, 5, ierit 7, 2, 4;

rat 10 "

erb).

(1) First conjugation: all in the 3rd. pers. sing. except existimarint 3, 8, 1; cogitarit 10, 6, 4; degustarit 12, 2, 4; honorarit 5, 10, 118; militarit 7, 4, 4; mutarit 5, 4, 2; notarit 9, 3, 47; recitarit 10, 5, 13 (eight verbs).

10, 5, 13 (eight verbs).
(2) Third conjugation, only three verbs; concupierint 10, 2, 14; consuerint 11, 2, 88; noris 4, 2, 22; norit 2, 1, 4: 5, 7, 7: 11, 3, 180; norimus 10, 1, 10 and norint 1, 1, 25.

(3) Fourth conjugation. In the 1st pers. only audierim 12, 5, 5; petierim 6 proem. 14; scierim 12, 11, 8; in the 3rd pers. sing. perierit 7, 2, 8; petierit 7, 4, 4: 8, 5, 14; scierit 4, 5, 1: 5, 10, 6: 7, 10, 6; and plur. audierint 2, 4, 15 (seven verbs).

(b) Pluperfect, chiefly in the verbs of the

first conjugation.

(1) First conjugation. In the 1st pers. only adprobassem 3, 6, 63; dubitassem 6, 5, 1; in the 2nd pers. only repudiasses 8, 5, 31; in the 3rd pers. sing. twenty-two verbs: alienasset 5, 10, 17; amasset 10, 1, 130; celebrasset 11, 2, 12; conciliasset 5, 10, 17: 11, 1, 9; desperasset 5, 12, 13: 6, 3, 84; elaborasset 2, 3, 6; errasset 11, 2, 38; exclamasset 6, 3, 81; iactasset 11, 3, 129; indicasset 4, 1, 67: 8, 2, 2: inquinasset, 10, 1, 100; interrogasset 5, 11, 3: 12, 10: 6, 3, 84: 12, 10, 57; intrasset 8, 3, 67; negasset 5, 12, 10: 11, 1, 28; occupasset 8, 4, 5; postulasset 11, 2, 50; probasset 10, 1, 56; spectasset 2, 20, 3: 6, 3, 71; superasset 11, 1, 40; terminasset 12, 2, 23; vacasset 10, 1, 44; vapulasset 9, 2, 12. In the 3rd pers. plur., 5 verbs: durassent 5, 11, 41; evigilassent 9, 4, 12; excitassent 4, 2, 58; pugnassent 5, 7, 27; putassent 10, 2, 4.

(2) Third Conjugation, only one example,

nosset 12, 10, 57.

(3) Fourth Conjugation, 3rd pers. sing. only two examples: sepelisset 8, 5, 16 and scisset 1, 10, 48; 2nd pers. plur. only one: audissetis 11, 3, 7.

It is to be noted that while norit is used

in 2, 1, 4: 5, 7, 7 and 11, 3, 180, noverit occurs in 4, 2, 20; cf. also norint 1, 1, 25 and noverint 1, 4, 13; norimus 10, 1, 10 and noverimus 1, 7, 1 and 12, 9, 19.

Quintilian's usage may be summarized as

follows :-

(1) Contracted perfect forms occur in all 185 times, most often in the first conjugation, 123 times, 66 per cent., and next to this 37 times in the fourth and 25 times in the 3rd. Contract forms are not found in the second conjugation.

(2) With regard to Modes, contractions occur most often in the subj., 69 times, next in the infin., 66 times, and in the ind.

52 times.

(3) With regard to the Tenses, contractions occur most often in the pluperf. (subj.) 43 times, perf. (ind.) 33 times, and perf. (subj.) 26 times, and least often in the fut. perf. 11 times and pluperf. (ind.) 8 times.

perf. 11 times and pluperf. (ind.) 8 times.

(4) With regard to the *Infinitive*, verbs are contracted most often in the first conjugation, 45 times, and in the fourth conjugation only 11 times, and in the third conjugation only in the verb nosse, 10

(5) With regard to the number of different verbs, 47 are contracted in the infinitive, 42 in the subj., and 37 in the

ind.

Though not belonging to this category, it may be noted in this connection that Quintilian discusses also the forms of the perfect in -ere in i. 5, 42 fol., rightly rejecting the explanation of it as a dual, which some grammarians advocated, but incorrectly explaining it as a weakening of runt. Cicero, Orat. 47, 157 declares in favour of the form in -runt, but the form in -ere is common in the poets (cf. e.g. Vergil Wotke Wien. Stud. viii.), and in the poetical prose of the Silver Age. So Quint. uses this form 6 times, dixere, 1, 5, 43; fuere 2, 16, 5: 12, 10, .10; indulsere 10, 1, 84; proposuere 1, 8, 12; and successere 3, 1, 13. In the same passage Quint. also gives an incorrect explanation of the forms in -re of the 2nd. pers. sing. pass., similarly explaining it as a weakening of -ris. He uses the form in -re but once, utare in 2, 1, 12.

EMORY B. LEASE.

THE CODEX TURNEBI OF PLAUTUS AND THE BODLEIAN MARGINALIA.

In the volume of the Classical Review for 1897 (pp. 177, 246) I gave an account of a Bodleian copy of Plautus, in whose margins has been entered a set of remarkable readings for a portion of the plays, and I stated reasons for believing these readings to have been taken from that famous lost MS., known as the Codex Turnebi (T), which was used by Adrien Turnèbe in the sixteenth century. This identification of the Oxford marginalia with a collation of T was so favourably received by the leading Plautine scholars of the continent, that when the Delegates of the Clarendon Press generously undertook the publication of collotypes of the marginalia along with a few pages of prefatory matter (The Codex Turnebi of Plantus, Oxford, 1898), I preferred to use the space at my disposal in a discussion of the relation of these marginalia to the 'libri veteres' of Lambinus and the 'vetus codex' of Scaliger, rather than in a fuller statement of the evidence connecting these marginal variants with Turnèbe's MS. That the good readings of Scaliger's 'vetus codex' are nothing more or less than these actual written entries in the Bodleian volume is, I hope, satisfactorily proved. The proof that those 'libri veteres' of Lambinus which contained good readings were merely marginalia, similar to the Bodleian, and not actual MSS., is not quite so cogent, but is, I think, too strong to be set aside without the production of weighty evidence in support of any other hypothesis. These variants however of Scaliger and Lambinus are of comparatively small mo-The Codex Turnebi on the other hand is of paramount authority for the text of Plautus, and it is of the utmost importance that Plautine students should be able to know definitely and without reservation whether there is or is not adequate justification for believing these Bodleian marginalia to represent the actual readings of the lost codex. Now that Prof. Sonnenschein has given expression to some doubts which have suggested themselves to his mind, I gladly avail myself of the opportunity of re-stating the evidence with the help of the fresh material which has accrued since the discovery of the precious volume in the Bodleian.

First let me give an account of the volume itself. It is a copy of the Gryphius edition of 1540, of small octavo size. The written marginalia, which often seem to suffer from lack of room in the narrow margins, are by

the hand of François Duaren (1509-1559). a friend of Turnèbe, known to us as an eminent authority on Law, but hardly as a They have not been classical scholar.1 taken by him at first hand from MSS. of Plautus but have been transcribed en bloc. whether mediately or immediately, from a collation, made presumably by some one else. The numerous mistakes in transcription are a proof of this e.g. p. 502 (ad Pseud. 1175). S. ceminus malum for Scena m.m. For etiam we often find et (see my note on Pseud. 783); an o of the original is often miscopied as r (see my note on Pseud. 816). Here and there a variant has been misplaced; e.g. the variant non aeque scitus which belongs to Pseud. 745 has been wrongly entered opposite v. 748 where the word scitus in the text caught the eye of the writer.2 And

¹ It was not till after the appearance of the Classical Review articles that I learnt from M. Léon Dorez of the Bibliothèque Nationale, a gentleman whose knowledge of the handwriting of the sixteenth century scholars is unrivalled, that the entry on the fly-leaf: Hae notae in margine sunt manu Francisci Duareni Juriscons. celeberrimi ex veteri Codice, came from the pen of Joseph Scaliger, a younger contemporary of Duaren. Being unable to find at Paris or elsewhere any specimens of Duaren's handwriting, I had previously felt some hesitation in regarding this statement on the fly-leaf as certain proof that the marginalia were written by Duaren. When Prof. Sonnenschein says 'I fancy I detect evidence of a distinct hand in some places,' he is, I suppose, referring to one or two entries by Scaliger (or Nicholas Heinsius, I cannot be sure which), such as these on p. 493 (see the collotype): si credis, nume. si non, ne mina quidem (Camerarius' emendation of Pseud. 877), condiam (ad v. 889), to which I have called attention in my notes. There are in the earlier part of the volume a few entries by a third party (perhaps the poet Remy Belleau or his friend and brother-poet, Étienne Tabourot, both of whom were successive owners of the volume), e.g. 'ung pot a vin' (ad cirneam, i.e. hirracm, Amph. 431). But these are so few and so trivial as to be unworthy of mention. That the whole body of marginalia is written by one person is unmistakable; and with the explicit statement of Scaliger before us, we can hardly venture to doubt that that person is Duaren.

² This transposition (there are many similar cases) has led Prof. Somnenschein into the mistake of supposing that v. 748 has two variants from a source indicated by a symbol to be discussed later, these two being (1) non acque scitus, which, as we have just seen, belongs really to v. 745, and (2) scitus, which is the proper variant for the line. Duaren's handwriting, though not nearly so illegible as Turnèbe's, is not always of the clearest, so that it is not surprising that Prof. Somenschein should have read the symbol in its first occurrence as dn. But a closer examination of the different ways in which Duaren forms the various letters, makes it certain that the letter here is r and not n. So the statement to

very frequently a variant which stood in the ALTA. inner margin of the original has had to be transferred to the outer margin 1 of the copy, -1559). owing to the narrow limits of the inner as an margins of the Gryphius edition of 1540. lly as a I am unwilling to take up more space in acbeen cumulating proofs. Anyone who will take MSS. of the trouble of examining the collotyped en bloc. pages will be able to satisfy himself that from s Duaren's marginalia are a copy and not an ne else. original. In the Bibliothèque Nationale I ion are found a transcript of Duaren's marginalia . 1175). (entered in a Gryphius text of 1535) which or etiam I have found of great use in suggesting 1. 783): means of inferring the nature of Duaren's ed as r original from various peculiarities of the ere and copy. The original of which Duaren made e.g. the a transcript seems to have stood in much the same relation to Duaren's transcript as ongs to red op-Duaren's transcript itself to the Paris rein the production. Indeed I will furnish later some reasons for believing the original to have been marginalia not merely in a the Clasprinted edition but in a Gryphius edition, either of 1535 or 1537. All that I am entleman going to say about what Duaren has written the six-

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posed original. What then is the nature of these marginalia of Duaren, and (by inference) of the marginalia (belonging, I fancy, to a Gryphius edition either of 1535 or of 1537) which he was at the pains of transcribing? It was not till after the articles in the Classical Review had been written that I had the means of giving a full answer to this question. In a lucky moment I came across a British Museum MS. of Plautus (referred to 'the latter part of the fifteenth century'), known as the Codex Burneianus (Burn.), a MS., which, like all 'Renaissance' MSS., contains the worthless 'Italian recension' of the text. The Duaren marginalia represent in the first place a collation of a transcript (I will call it Burn.2) of this Codex Burneianus.2 The Burneianus has in the two earlier

will apply 'mutatis mutandis' to this sup-

Pseud. 748 we find different readings signed respectively dn, dr' is erroneous. Both are signed dr, and the first of them refers to v. 745, not to v. 748.

¹ That, by the way, seems the explanation of the

That, by the way, seems the explanation of the intrusion into the left-hand or outer margin on p. 488 of the variant for Pseud. 730 Caristo. It had to be squeezed into an open space in the centre of that entry Ex fragmentis, &c., which I mention below.

The proofs of this relationship cited in the Appendix to my Codex Turneb are, It rust, such as can leave no room for doubt. It is possible that the

leave no room for doubt. It is possible that the transcript still exists in some library, public or private. But neither a printed circular sent to public libraries in France and elsewhere, nor an article in the *Ilevue de Philologie*, have as yet succeeded in eliciting any clue to its existence. And really the Codex Burneianus supplies us with all that we need. plays, and here and there elsewhere, a copious marginal commentary containing references to Nonius Marcellus, Paulus'epitome of Festus, etc., along with other elucidations of Plautine terms. These have been tranof Plautine terms. These have been transcribed 'in extenso' in the collation, so that the Duaren marginalia are of considerable extent in the early pages of the Bodleian The various readings too which are noted in the margin of the Burneianus have been faithfully copied by the collator, e.g. at Men. 75 alibi in alio codice invenitur textus sequens, etc., at Amph. 342 al(ia)s qui pugnis os exossas hominibus. These however are not numerous, so that the Duaren marginalia soon become of smaller extent and leave ample space on the margins.

In the middle of the Pseudolus (at about v. 730, on p. 488 of the Gryphius edition) all this is changed. The pages previous to p. 488 are not included among the collotypes, but if one looks at pp. 612-615, the last collotyped pages of the Rudens, one will get a notion of the appearance of the pages that precede p. 488, with their marginal spaces but little occupied by the scanty collation of Burn.2. The scantiness of the collation had made it possible for the collation of another MS. to be entered in the same margins. And so, when we turn from p. 487 with the usual slight equipment of marginalia, we find a quite different state of matters on p. 488 and the following pages. The few variants from Burn.2 appear in company with a large set of other variants. And while these variants from Burn.2 had previously no symbol attached to them, they are now followed by the symbol poict. (proict., pict.). This symbol is used to distinguish them from the variants which come from another source, these latter variants being provided with a symbol of their own, which looks like dr. (with a horizontal stroke above), and of which the first letter was certainly taken by Duaren for d, since it has sometimes the 9 form, as well as the ordinary d-form (with shaft intersected by the horizontal supra-The second letter is ocscript stroke). casionally o and u. At the top of the page is the entry in somewhat large writing (at least in the opening lines): Ex fragmentis | monast(erii) s(anctae) | columnae se | non(ensis) urbis | Adriani Tor | nebi jubas. | lubas, Poict. | juuas alta | manu, an entry which clearly does not faithfully represent the form of its original, for columnae is a mistake for Columbae. Benedictine Monastery of Sancta Columba (Sainte Colombe, a virgin-martyr of the

third century A.D.) at Sens (the Latin name of which is urbs Senonensis or urbs Senonum) was, like other Benedictine monasteries, a home of learning, as we know from the letters of Bishop Lupus of Ferrières to Altuinus, a monk of this establishment in the ninth century. It was pillaged and burnt by the Calvinists in 1567, when these 'fragmenta' probably perished in the destruction of the library. Into the middle of the entry has been squeezed the variant for 730, viz., caristo, a variant which, I have already suggested, may have stood in the

other margin of the original.

These symbols poict. and dr. are entered on this page conscientiously enough, but are very soon discarded. For a time the rule is followed, more or less consistently, of using them when a pair of variants are noted for the same word or phrase; e.g. at v. 778, where praebitere stands in the Gryphius text, we find in the margin perdere poict. and (below) perbitere dr .. Even this is found to involve too much trouble, and the use of the symbols becomes quite exceptional, the only visible clue to the source of the one and the other variant being the order in which they are written. The variant from Burn.2 normally stands before the other. All this is so natural a proceeding on the part of a not over-careful copyist that it would be absurd to try to find some special significance in the presence or absence of the symbols in Duaren's transcript, or to argue that we are entitled to refer to one and the same source only such variants as happen to be provided with the symbol dr., or only such as happen to be provided with the symbol poict.

The excellence of these new variants which now appear side by side with the variants from Burn.² is happily beyond dispute. It will be sufficient here to mention four: (1) Poen. 977 supplying the latter half of the line (Punicast guggast homo), which is omitted in all MSS. except the Ambrosian Palimpsest; (2) Poen. 943 (in the Carthaginian passage) luful, the true form, preserved by the Ambrosian Palimpsest, where the other MSS. have lueui; (3) Poen. 1355 had uerbum, where the minuscule MSS. have adversum and the Palimpsest correctly haud uerbum; (4) Pseud. IV. ii. (in the scene-heading) C, i.e. Canticum, a symbol found in no other

MS. here.

Where can these variants have come from? We know that there was in existence in France in the sixteenth century a fragmentary MS. of Plautus of considerable age, and containing an extremely good text. It is mentioned several times by Adrien Turnèbe (Professor at Paris from 1547 to his death in 1565) in his Adversaria, a huge mass (in thirty volumes) of emendations and explanations of passages of Latin authors. Prof. Goetz, in the Introduction (pp. vii. sqq.) to his edition of the Poenulus (Leipzig, 1884), collected Turnèbe's notices of the MS. and its readings, and from an investigation of them drew the conclusion that this Codex Turnebi (T) belonged to the 'Palatine' family of Plautus MSS., the family to which our minuscule MSS. (the best of which is B, and the next best C and D) belong, rather than to the 'Ambrosian,' of which the sole representative is the Ambrosian Palimpsest at Milan (A). He showed further that it must have exhibited the 'Palatine' text in a very superior form; for it agrees with B, the best existing representative of the family, in passages where CD go wrong, and on the other hand in passages where B goes wrong with CD, it preserves the true reading, which, of course, is often confirmed by A. Here are his words: 'Fuit autem liber optimae notae. . . Verba in BCD omissa, integrum versum, qui in CD deest, conservat, cum A saepius consentit, in egregiis scripturis diversa BCD librorum memoria, saepissime cum B, discrepantibus CD. . . Certe codex Palatinorum recensioni adscribendus est, non palimpsesti. . . Fuit igitur Palatinae recensionis codex praestantior certe multis locis-quoniam consentaneum de propriis vitiis Turnebum non rettulisse-quam ei libri qui nunc praesto sunt.' These conclusions, so far as I know, have not been challenged by any one except Prof. Leo, and he has now withdrawn his opposition (Deutsche Litteraturzeitung, 1898).

The new variants in Duaren's marginalia conform precisely with this description. They agree with BCD against A, except in some passages where BCD have gone wrong (e.g. Poen. 977, 943 quoted above), and with B against CD. Further, they offer a number of new readings which are manifestly the readings of the protoarchetype of the Palatine family, e.g. Poen. 1355, quoted above. Such readings evidently come from an excellent source. Can we be sure that that source was the Codex Turnebi i 'A priori' there is considerable likelihood that it was; for it is impossible to imagine it to have been the Ambrosian Palimpsest, in which the text of Plautus lay concealed under a text of the Old Testament, until at the beginning of this century

Cardinal Mai, by the application of chemicals, revived the old writing; nor is it safe to imagine that there were more first-class MSS. of Plautus in existence in the sixteenth century than those now extant and the one of which we know by report. 'Renaissance MSS.' of Plautus were indeed common, but when we consider the excitement produced by the discovery in the previous century of the eleventh century MS. (D) and the number of students in Turnèbe's time who were eagerly employed in the emendation of the text of this favourite author, it seems improbable that any MS., whose antiquity would mark it off from the current MSS. of Plautus, would be collated by any scholar without the fact being di-vulged to the learned world. Would that it were true that a number of such MSS. were then in existence! There would be a possibility of their survival now in the shape of leaves used for binding purposes, if in no other. But I am afraid the chances are strongly against such a sup-

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Let us compare one by one Turnèbe's quotation of readings from his codex with the Duaren variants: (1) Pseud. 738: 'quod in schedis quibusdam veterrimis repperi...in quibus scriptum est hircum ab alis; and elsewhere 'in pervetusto legi hircum ab aliis.' The Duaren entry is 'hircum ab aliis dr Is cum ab aliis poict. sed ab "aliis" un [sic] i "axillis"; (2) Pseud. 743: 'in veteribus illis, quas dixi, schedis scriptum erat lam-The Duaren entry is jamberas proict., lamberas; (3) Pseud. 1051 (a line omitted in CD and the early printed texts as in Burn.): 'repperi et in antiquis membranis ...unius versiculi auctarium... Ite hac tri-umphi ad cantharum recta uia.' The Duaren entry is Ita ac triumphi and nothing more; (4) Pseud. 1100: 'in iisdem pergamenis ad molas coloniam.' The Duaren entry is ad molas colonias (the variant from Burn.2 The Gryphius text has coloniam); (5) Poen. 19: 'recte in membranis scriptum erat Neu designator practor os obambulet.' The Duaren entries are dessignator (Burn. has dissignator) and praeter os (Burn. has praeteriens) obambu-The Gryphius text has Neu; (6) Poen. 30: 'illae quoque membranae scribebant Ne et ipsae sitiant et pueri peritent fame.'
There is no Duaren entry. The Gryphius text has Ne et ipsae sitiant et pueri pereant fame; (7) Poen. 47: 'aliquando in aliquot membranas pervetustas incidi, in quibus scriptus erat Poenulus, cuius in prologo scriptum erat gnarures.' The Duaren entry is aequi mecum sitis signatores. aeque mecum

sitis aegnariores, the first version being that of Burn. 2; (8) Poen. 55: 'et in eis item commode Nomen iam habetis nunc rationes ceteras.' There is no Duaren entry. The Gryphius text has Nomen iam habetis. nunc orationes ceteras; (9) Poen. 137: 'mihi membranarum quarundam perveterum auctoritas fidem facit lyrae lyrae germanum esse. There is no Duaren entry. The Gryphius There is no Duaren entry. The Gryphias text has $\lambda \hat{\eta} \rho o \iota \lambda \hat{\eta} \rho o \iota \gamma$; (10) Poen. 231 (scimus facere, etc.): 'mihi videbatur Plautus "ennam" usurpasse prima statim specie hoc in versu...sed in membranis pervetustis repperi en iam.' The Duaren entry is Scimus facere | nniam | followed by Scimus facere. The latter version (i.e. with the omission of the word) is the reading of Burn.2; (10) Poen. 319: 'in membranis vetustis repperi prima cum ut.' There is no Duaren entry. The Gryphius text has primae ut; (11) Poen. 371: 'in iisdem membranis erat ego faxo Si non irata es ninnium pro te dabit...sed puto tamen scribendum esse "numulum pro te dabit." The Duaren entry is Ego faxo si non irata es nimium pro. The Gryphius text ends the line with nimium pro te dabit, and so does Burn.; (12) Poen 413: 'sic e veteri membrana lego maiorem partem minore habitas mea. The Duaren entry is maiore parte | minore | habita mecum [or perhaps meam], and the major of the text is corrected by suprascript 'e' to maiore. Since meam is the reading of Burn. (majore parte minore habitas meam), I am inclined to regard this as a poict.-variant; (13) Poen 471: 'ex illa vetusta membrana, aliquantulum tamen scriptura conturbata, lenulle de illa pugna pentethronnica.' The Duaren entry is lenuile de illac, and the Gryphius text has pugna pentethronica. The reading of Burn. is lemni and penetronica; (14) Poen. 478: 'in membranis illis legebatur... farferi.' The Duaren entry is farferi (the farfari of the Gryphius text being corrected by suprascript e). This is the reading of Burn. also; (15) Poen. 530: 'in membranis pervetustis repperi et clauatorem,' and again, 'tantum tamen tribuo illis aliquot membranis quas aliquando habui, ut earum lectionem bonam testari et interpretari debeam, uel clauatorem gradu.' The Duaren entry is glabatorem | clabatorem, the former word being the reading of Burn.² The Gryphius text ends the line with gradu; (16) Poen 580-1: 'in utroque versu "condoctum" legimus, non "conductum" et membranis adstipulantibus et favente sensu.' The Duaren entry at v. 580 is condocta (the conducta of the Gryphius text being corrected by suprascript o);

at v. 581 the indoctior of the Gryphius text is left unaltered. The reading of Burn. is in v. 580 conducta, in v. 581 indoctior; (17) Poen. 770: 'scribo etiam ex earum fide peregrinum Spartanum id nunc his cerebrum uritur. The Duaren entry is apparently his cerebrum utitur, though the first t of the last word shows an unusual form. Burn. reading is id nunc hisce credo auditum. The Gryphius text begins the line with Peregrinum Spartanum id nunc; (18) Poen. 778: 'scribo etiam ex earum fide nego et negando siquid refert rauio.' There is no Duaren entry. The Gryphius text has Nego et negando si quid refert aio ; (19) Poen. 977: 'in iisdem membranis exaratum facies quidem aedepol punica est, guggast homo.'
The Duaren entry is aedepol punicust guggast homo dr. The Gryphius text begins the line with facies quidem; (20) Poen 1033: 'ego in pervetustis membranis repperi...micdilix.' The Duaren entry is nundilia | migdilia, the former word being the reading of Burn.2; (21) Poen. 1168: 'legendum puto graecae sunt hae columnas sustolli solent, subscribente huic lectioni veteri libro.' The Duaren entry is: Thecoe sunt celonnae sustolli solent. Threcae sunt celumnae sustolli solem, the former version being that of Burn.2; (22) Rud. 363: 'ita in libro antiquissimo scriptum repperi...anancaeo.' The Duaren entry is amancaeo do annaneo ananuo netiam. (The last words seem to be miscopied; perhaps for 'uno n.' Burn. has To these explicit citations of the ananeo). 'Codex Turnebi' we may add the following, in which there can hardly be room for doubt that the same MS. is referred to; (23) Rud. 613: 'integratum...inveni versum hunc, qui vel mutilus vel nullus est in exemplaribus, sed quid hic in Veneris fano meae viciniae.' The Duaren entry is Sed quid hic in Veneris fano meae uiciniae (with uiae on the other margin). Burn.2 had merely Sed quid hic in Veneris fano, with omission of meae uiciniae; 1 (24) Rud. 724: 1 (lego non licet est lex apud nos. The Duaren entry is Non licet ita est lex. The Gryphius text has merely Est lex apud nos and so had Burn.2

It will be noticed that Turnèbe's citations do not always furnish us with the 'ipsae litterae' of the readings in his codex. For the emendation lenulle in Poen. 471 he ex-

¹ Goetz seems to be perfectly right in adding to this list other readings from the Adversaria in which the reference to the Codex Turnebi is not so unmistakable. But it will be well here for purposes of argument to omit any reading which might be shallowed.

challenged.

pressly adds to his citation the qualifying clause 'aliquantulum scriptura conturbata, a description which suits the Duaren variant lenuile. In justifying clauatorem in Poen. 530 against the rival reading grallatorem, his words cannot be construed to mean that clabatorem was not the actual spelling of his codex. If a modern editor were appealing to the support of a MS. for, let us say, the proposal to read Phoebus instead of Poenus, he would surely make no scruple of citing its testimony in his favour, even though it showed the ordinary mediaeval spelling febus. Even the remark on Poen. 231 does not seem to me quite to exclude the possibility of facerenniam having been the actual form offered by the Codex Turnebi. Above all, where Turnèbe says merely that his MS. 'favours' his emendation we must be cautious against unduly pressing his words (e.g. Poen. 1168, Poen. 581). In fact, the need of caution is impressed on us by the very fact that in two cases in the above list, in which Turnèbe refers more than once to the same reading, his statements are not wholly consistent. The reading for Poen. 738 is in one passage given as hircum ab alis, in another as hircum ab aliis; the reading for Poen. 530 as et clauatorem and uel clauatorem. But after all these allowances have been made, we still have quite sufficient material for identifying a collation of the Codex Turnebi (T). How do the drvariants stand the test?

With all the characteristic readings of T, the readings which mark it off sharply from other MSS. these variants coincide: (1) the portions of lines which are omitted by all the other Palatine MSS. (viz. Poen. 977 punicast (-cust) guggast homo, Rud. 613 meae uiciniae, 724 non licet), or which are omitted by all but B (viz. Poen. 1051 Ite hac (ac) triumphi etc.), (2) the correct readings where all the Palatine MSS. have gone wrong, viz. Poen. 770 his cerebrum uritur (ut-) [hisce crebro auritur CD, om. B], Poen. 1033 mic-dilix (migd-) [micdilia BCD]; or all but B, viz. Pseud. 738 hircum [hiscum CD], Rud. 363 anancaeo [ananaeo CD], etc. absence of a T-reading from Duaren's marginalia is of course no proof that they do not contain a collation of T. The collation transcribed by Duaren probably does not represent the full, final collation made by Turnèbe (see below); and Duaren himself has probably omitted a good deal that was in his original. At Poen. 1051 he has copied only half of the supplementary line, Ite ac triumphi, being possibly at a loss to decipher the ad cantharum of his original;

for it is hardly conceivable that his original contained only this fragment. His omission of the variant rationes at Poen. 55 would imply merely his overlooking a dot under the first letter of the word printed in the Gryphius text, orationes, and so on. The only serious discrepancy seems to me that at Poen. 47, where Turnèbe quotes from his MS. gnarures (possibly spelt ignarures, as in the Palatine archetype), but where the dr-variant is aegnariores. This is so extraordinary a form that I suspect it to be due to miscopying by Duaren. The recurrence of the initial syllable as in the entry that he was transcribing may in part account for his blunder: aequi mecum sitis signatores, aeque mecum sitis aegnariores. Indeed this is the great defect of these Duaren marginalia, the carelessness shown in transcription.1 On the other hand Turnèbe's confession of the haphazard condition of the papers from which he compiled his chapters and of the trouble that his handwriting gave to the printers (see my Codex Turnebi, Introd. p. 12), shows us the danger of regarding as infallible all the statements in the Adversaria, especially as a great portion of it was published after Turnèbe's death.

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The agreement of the dr-variants in Duaren's marginalia with what Turnèbe's statements enable us to know of the actual readings of T seems to me as perfect as under the circumstances could be looked for. To the majority of Plautine scholars, I fancy, the full details I have given above will seem superfluous. It will be enough for them to know that the dr-variants (1) include the important test-readings, Poen. 977, Rud. 613, 724, Poen. 770, 1033, (2) contain new readings that were manifestly the readings of the Palatine archestly

type, (3) agree with the best tradition of the Palatine text.

But, it may be asked, are we justified in regarding all these readings (all, that is, with the exception of the Burn.2-variants) as having come from the Codex Turnebi? The good Palatine MS. B was (along with C) brought to light by a contemporary of Turnèbe, Camerarius; and although Camerarius' edition of Plautus did not appear till 1552, perhaps after the Bodleian marginalia were penned, and his notes in that edition give but scanty information about the actual text of the MS., still some knowledge of its readings may well have been communicated to Turnèbe, by the German scholar. And may not a number of the marginal entries be really emendations made by Turnèbe himself or by contemporary scholars, and not actual readings of the Codex Turnebi or of any MS. 2 Professor Sonnenschein states the case strongly: 'It would be strange indeed if the readings of B and C, recently discovered by Camerarius, did not arouse interest in the minds of scholars of the day like Turnèbe... Is it not a large assumption to suppose that he closed his columns to any readings except those of one favourite codex and of a worthless MS. of the Italian recension?' He would have made his statement still stronger if he had added to it that remark of Lambin on Turnèbe's method of work, which is quoted on p. 12 of my Introduction: cum ille quippiam ingeniose ac praeclare excogitarat, in suo codice [i.e. printed copy of Plautus] perscribebat. Cum aliquid item ab altero ingeniose et erudite inventum reppererat, id in suum codicem referebat, tacito interdum auctoris nomine, quod properaret, non quod quemquam sua laude fraudare vellet. Here we have definite information from a younger contemporary of Turnèbe that that scholar had a habit of jotting down in the margin of his copy of Plautus contributions from all kinds of sources without adding satisfactory symbols of discrimination. At first sight this looks like a strong confirmation of the suggestion that these marginalia may be after all 'a heterogeneous collection of variants, not from any one MS. but from several (including one or more of the Palatine MSS.), and of comments derived from various sources.'

¹ Still I am less inclined to censure Duaren, now that I have had opportunity of examining Turnèbe's handwriting. The best that can be said of it is that it is not quite so illegible as Lambin's. Duaren had a far more difficult task than the writer of the Paris transcript of these Bodleian marginalia. And yet, if we compare the two, I do not know that Duaren's mistakes greatly outnumber the other's. For all that, such mistakes as (on p. 488) columnae for Columbae, un i for uno i, alta manu for altera manu (again on p. 498 castigatus alta nu for castigatus altera manu) are provoking at the outset of a transcript of so important a collation. The alta, by the way, is more likely to represent altera than alia; for if altera were written with the contraction of ter, viz. t with a cross-stroke intersecting the upper part of the shaft of the t, this contraction would be very easily confused with that by-form of t, which has become with us the current form. Prof. Sonnenschein's remarks about alta manus and his reference to the mysterious altana scriptura, mentioned by Ducange, are hardly to be taken seriously.

² Those who would refer a correct reading in a drentry to a conjecture, say of Camerarius (e.g. Rud. 738), will have to explain why a wrong emendation of Camerarius never occurs in these entries. At Rud. 614 it is worth mentioning that the entry is animus miratur, not animus miratur meus.

My reply is that the original of the Bodleian copy was not a copy of Plautus that would suit Lambin's description. It was not the copy (or one of the copies) which Turnebus kept by him as a storehouse for emendations and elucidations of the text. A first examination of it does indeed suggest this as its character; for the early pages have their margins filled with quotations from Nonius Marcellus and Paulus' Epitome of Festus and with exegetical notes. But these, as we have seen, all come from the margins of Burn.2 They are merely a part of the collation of Burn.2 The original of the Bodleian copy was a text used for purposes of collation merely; and Turnèbe would, no doubt, have several texts of the kind, each containing in its margins the collation of one MS. The scantiness of the collation of Burn.2 made it possible, as has already been remarked, to utilize the same volume for the collation of a second MS., the fragmentary Codex Turnebi. The occasional remarks, and even (once or twice) quotations of passages of ancient authors, that are found in the marginalia, are all such as would naturally be suggested at the moment of collation; e.g. at Pseud. 962 Quartum has aedes. | Quotumas aedis dr. Quotumus ut in 'quotimo die,' Scena, 'malus' (with reference to v. 1173, in Act IV. Sc. vii., the scene which begins with the words 'Malus et nequamst homo'). That this is the true character of the Bodleian volume (or rather of its original) is perfectly clear. The only form in which a heterogeneous composition of these marginalia could be posited so as to square with the facts would be to say that, while the poict-readings are taken from an actual MS. (Burn.2), the dr.-readings come from written marginalia in a copy of Plautus which did answer the description of a 'storehouse of variants and conjectures.' These readings, we should have to suppose to be taken from (1) T itself, (2) B (possibly also C and D), (3) the conjectures of contemporary scholars or of Turnèbe himself. Put in this form, an attack on the identification of the dr.variants with a collation of T is at first sight somewhat disconcerting. For undoubtedly this hypothetical triple origin will account for all the dr.-variants. Those of them which we know to have been peculiar to T can be referred to T itself; those which are also found in B can be declared to have emanated from that actual MS. (i.e. from information supplied by Camerarius); while, since the possibilities of conjecture are unlimited, we can conveni-

ently refer to this source all the correct readings which now for the first time become known to us, or which have been independently stumbled upon by recent scholars or which are found in the Ambrosian Palimpsest. The intractable remnant, naturally a small remnant, namely such readings as are obviously right and yet which cannot be due to conjecture (e.g. luful Poen. 943) we can get rid of by allowing them also to come from the Codex Turnebi. To characterise the dr.- variants in this way hits our theory hard, although we may comfort ourselves with the reflection that after all that is exactly the way in which a collation of T would lay itself open to be described. For the characteristic feature of T was, as indicated in the remarks of Prof. Goetz quoted above, its agreement with B, the best representative of the Palatine text, except in places where B (or more often the immediate original of BCD) had fallen into error. The very virtue of the dr.-variants, their exact conformity with all that we should imagine a collation of T to be, is what renders them liable to an attack of this kind. On the other hand, they may be said to be saved by a defect of theirs, their fragmentariness. If they came from a source of the kind suggested, why should they begin at Pseud. 730 and end in the middle of the Rudens ! This limited range of theirs suits the theory that they represent the collation of a fragmentary MS., but will hardly suit any other theory. No! I think we may feel ourselves justified in regarding the dr.-variants as of the same character as their neighbours, the poict.-variants. Both sets represent the collations of actual MSS., but the MS. whose collation furnished the dr.-variants was a fragmentary MS., unlike Burn.2 And since the appearance of the collation of this fragmentary MS. is heralded by the entry Ex fragmentis, etc., the true form of which, though somewhat obscured in Duaren's transcript, can be recognized as Ex fragmentis monasterii S. Columbae Senon. urbis Adriani Tornebi, we may reasonably conclude that in this entry we have the heading with which Turnèbe prefaced his collation and that the fragmentary MS. collated was the Codex Turnebi itself.

The knowledge of the meaning of these symbols, poict. and dr, would certainly be welcome. At the same time it is not indispensable. It is enough to know that the one symbol indicates a collation of $Burn^2$, the other a collation of T. In my articles in the Classical Review I suggested

that dr. might mean 'Duaren.' suggestion was made under the mistaken idea that Duaren might have been the transcriber of the original of the Bodleian marginalia and not of these marginalia themselves. In my book I have suppressed this guess at the meaning of dr. and also my former explanation of poict., for I came to recognize the uncertainty of these hypotheses. In an Aldine text of Plautus in the Bibliothèque Nationale, about which M. le Breton wrote in the Revue de Philologie, xix. 255, a heterogeneous mass of marginal readings is found, a small fraction of which has come ultimately from the Bodleian marginalia. The poict.-symbol of the original has been whittled down to P. in the copy; 1 and M. le Breton was very naturally misled by a reference to Pithou (in a prefatory note in the volume) into the erroneous idea that P. stood for 'Pithoei' or 'Pithoeanus.' What dr. (or, for that matter, poict) may have been in Duaren's original, I cannot say with certainty; but since Prof. Sonnenschein has specially asked for it, I will not abstain from mentioning what seems to me at least a possible explanation.

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In one of the earliest occurrences of the symbol (p. 488, ad Pseud. 745) we have what looks like co dr., which may be co(dex) dr. Now, in the entry-heading the new variants are characterised at length as 'ex fragmentis, etc., Adriani Tornebi.' This description is obviously too lengthy to be repeated after each variant. It would be reduced, perhaps gradually reduced, by the collator. natural form of compression would be 'ex codice Adr. Tornebi,' or, with omission of the superfluous preposition and with the use of contractions for the other words, 'co. a. tr.' the 'a. tr.' being indicated by a monogram, dr., which had the appearance of d with a cross-stroke above followed by r, and which was so read by Duaren. Sooner or later even this symbol would be found capable of reduction and co. dr, would be replaced by dr., in which shape it was adopted by Duaren, with one relapse to the fuller co. dr. of his original. This provides at all events a natural transition from the full heading ex fragmentis, etc. Adriani Tornebi to the symbol employed by Duaren in all subsequent variants, dr.

To continue on the same ground of con-

¹ The Duaren entry at Pseud. 738, quoted above, appears in this misleading and incomplete form:

hircum ab aliis
Is cum ab aliis
P. sed ab aliis

jecture, rather than of certain inference, let us see what can be made out regarding the history of the Duaren marginalia. Since it was Turnèbe who was in possession of the MS., and since there is no record of any other scholar having been allowed access to it, it is natural to suppose that the collation must come ultimately from Turnèbe himself. Further, since Duaren was a friend of Turnèbe and in 1548 was for a time resident in Paris, where Turnèbe was Professor, the conjecture (for it is nothing more) is suggested that he received the collation from Turnèbe himself on that occasion. Still there is no reason why Turnèbe may not be supposed to have procured for Duaren for copying purposes, not his own collation, but a copy which had been made by, let us say, a pupil of his. If we suppose Duaren's transcript not to be an immediate copy of Turnèbe's collation, we may absolve Duaren from at least a portion of the errors of transcription. For my own part, I see no reason why we need posit an inter-mediary transcript. Prof. Sonnenschein's argument that Turnèbe would not talk of his MS. as the MS. 'Adriani Tornebi' does not appeal to me. MSS. were habitually designated after the scholar who owned them or who first utilized them, e.g. MS. Langii, codex Cujacii, etc. Turnèbe would naturally name his foundling 'codex Adriani Nor does the fact that the Tornebi. collation does not cover the whole of the fragments (for the Adversaria contain references to their readings in the Casina and elsewhere) indicate that this was not the collation made by Turnèbe himself. Why should it not be his first collation, made perhaps before he had received the full number of the fragments or possibly discarded before completion in favour of a collation made with some difference of method ? 2

Apart from 'a priori' considerations, however, we have some definite trace of Turnèbe's hand in these marginalia, as

The scattered readings in the Oxford marginalia from the Bacchides in addition to the continuous collation of Pseud. (latter half), Poen., Pers., Rud. (first half), are a curious feature. In the 'variorum' marginalia of the Paris Aldine mentioned above, there is a set of variants for the Bacchides alone. They come from a collation, made by J. Corbinelli, of D for this single play. Corbinelli chose the Bacchides, I fancy, because this was the opening play of the recently discovered portion of Plautus, viz. the last twelve plays. In what order the 'membranae' stood when they came into Turnebe's possession, we cannot say. Certainly the Duaren-marginalia make us imagine the leaves containing Pseud. 730-Rud. 790 to have occupied the first place.

much perhaps as we could look for in marginalia which are simply and solely records of the readings of two MSS., with only an occasional remark suggested at the moment to the collator. At Rud. 63 where the Gryphius text has auchit, while the Palatine Archetype had the corruption auenit (omitted by Duaren), there is a curious entry, so written as to give the impression that Duaren could not decipher his original and had to fall back on the resource of imitating as well as he could the 'ductus litterarum.' The entry looks like 'M pro S ut chodax pro cardax (cur-?) et ehigens [second letter doubtful] pro Lingens.' It puzzled me for a long time till I noticed in Turnèbe's Adversaria a note on a passage of Vitruvius, where he emends chodaces of a MS. to cnodaces ('pivots'). The true form of the first half of the entry is certainly N pro h ut chodax pro cnodax. Possibly a search through the Adversaria would reveal the true form of the other parallel quoted for the confusion of N and H in MSS. (perhaps lingens pro lingens); but at all events the coincidence that a note of this trivial kind, the mere record of an isolated manuscript corruption, should be found both in the Adversaria and in these marginalia is significant. Again at Rud. 753 the two readings offerrimentas (the dr- reading) and ferramentas (the poict .- reading) are followed by a note which justifies the former by an appeal to Festus' account of offerrumenta as called so 'quod offeruntur.' Turnèbe (Adv. xvi. 12) appears to have been the person who brought the line into connexion with this passage of Festus. I might add as another piece of evidence the note at Rud. 122 exsicasque harundinem, where the change of e to i in compounds of seco is maintained to be the old Latin spelling; for this is a characteristically Turnebian remark, repeated a surprising number of times in his printed writings.2 An expert in handwriting might perhaps find more convincing evidence of Turnèbe's authorship of the original of Duaren's marginalia. Certainly Duaren's confusion of o and r is easily understood from Turnèbe's penmanship; and I have noticed other features of the kind. But I prefer to content myself with the negative statement that, so far as I could see, there was no feature of Turnèbe's

writing that would not suit this hypothesis.

Another matter for which some scraps of evidence can be picked up is the form of Duaren's original. A transcript of collations that were entered in the margins of the printed text with which the MSS. had been collated would be most suitably made in the margins of the same edition of text; for a discrepancy in the printed text would distort the marginal collations. The transcript of the Bodleian marginalia which I found in the Bibliothèque Nationale, a transcript com-pleted in the year 1557, is entered in the margins of a Gryphius text (like the Bod-leian); not, however, of the 1540 edition, but of the almost identical edition of 1535. Where there is a divergence of text, at all events where it forced itself on the recognition of the transcriber, owing to the nature of the marginal variant which he was transcribing, the printed variety of reading is recorded by him as well as the written marginal variant of his original; and even though he had not expressly informed us, as he has done, that his original was an 'alter Gryphius' 'excussus an. 1540,' we might have managed to infer this from these notices of its readings. Now in the Bodleian marginalia we have one or two variants recorded which can hardly be supposed to have come from Burn.² (for a list of them see my Introd. p. 5). They are readings which characterise the Gryphius text of 1535 and 1537 in distinction to the edition of 1540. No other edition of Plautus, so far as I know, will suit the requirements of inference, so we may perhaps allow ourselves to infer that Duaren's original was a Gryphius text either of three or of five years earlier than the text which he himself employed.

The Paris transcript does not furnish us with any information about the Duaren marginalia, except that their date cannot be later than 1557. There is a second transcript, or rather partial transcript, of them in another copy of Plautus in the Bibliothèque Nationale, which was brought into notice by M. le Breton. In an Aldine edition of 1522, formerly owned by Passerat, that scholar has entered a limited selection from both the poict.-variants and the dr.variants, a selection which can be proved to come ultimately, if not directly, from the Bodleian volume. With regard to the immediate source of this set of variants, he gives us this information: Petrus Pithoeus this can only mean the famous P. Pithou, born 1539, died 1596] nobis commodavit

passag e from Turnèbe.

¹ I fancy the same thing occurs elsewhere, e.g. at Rud. 534 arerem. The Paris transcriber often adopts the same method with illegible entries of Duaren, e.g. this very entry at Rud. 63.

Lambin has clearly borrowed his note on this

Plautum emendatum a capite ad calcem hypocomparatione trium veterum librorum. These words I believe to refer to the Oxford raps of volume itself, though they may point to some transcript of it. If I am right, they orm of lations raise anew the question of the possibility of of the the dr.-variants not being derived from a d been single source, the Codex Turnebi. The in the poict.-variants we now know, thanks to the ; for a discovery of the Codex Burneianus, to be distort taken from one, and only one, MS. (Burn.2) t of the 'a capite ad calcem.' If then this statein the ment of the triple origin of the whole body t comof Duaren variants be true, it is the dr.in the variants that must account for two of the e Bodthree sources. The two sources to which these dr.-variants could be plausibly redition. 1535. ferred would be (1) T, (2) B. Here again , at all we have an attack on our theory which hits recogthe theory hard, for, as we have seen, that to the is precisely the description which suits the ich he dr.-variants. They agree with B, the best ety of representative of the Palatine text, except in cases of deflection on the part of B (and as the iginal; of course also on the part of T) from the sly inreadings of the Palatine archetype. So far riginal as the emendation of the text of Plautus is is an. concerned, we might be contented to acer this quiesce in this rival hypothesis; for, after Now in all, it matters little whether those readings or two of B, which are clearly the correct readings, e supor at any rate the readings of the Palatine r a list archetype, gain the additional support of T ey are or rest on the single testimony of B. yphius important contribution to the text of our to the author lies in the new readings in the lautus, Bodleian marginalia, the readings not atements tested by B; and these readings would, w oureven according to this rival hypothesis, be was a referred to T. Still, for the history of the years transmission of the Plautine text and for a imself number of kindred questions, it is important to get at the actual truth of what T conish us tained and did not contain. Let us for the Duaren moment admit the truth of Passerat's accannot count and see what it would involve. second should have to suppose the dr.-variants, ipt, of since they are plainly entered as variants in the from a single source, to have come from rought marginalia which combined a collation of B

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could stand. How then are we to explain the fact that the dr.-entries contain single readings, not pairs of readings? At Poen. 1033, for example, where Burn.2 had nundilia and T migdilix (micd-), while B has micdilia, why

and of T, and which are indicated in

Duaren's transcript by the symbol dr.

That is the only form in which the hypo-

thesis of a double origin of the dr-variants

is it that beside the poict.-reading nundilia we have as the dr.-entry merely migdilix? When we have pairs of readings, why is the one always a poict.-reading? Why should we not as often find a pair of dr.-readings, one the reading of T, the other the reading of B? And why should the entries go in pairs rather than in trios? The only way in which a supporter of this rival hypothesis could get out of the difficulty would be, I fancy, by the supposition that only one of the two MSS., T and B, was collated systematically, while the other was merely drawn upon for occasional readings. It might be said that T, being the fragmentary MS., would have the lesser part assigned to On the other hand we might reply that in France, the home of T, we should look for a full collation of so important a MS., while the difficulty of getting information about anything more than isolated readings of B, a German MS., would be very great. Though T was fragmentary, it can be shown on Turnèbe's authority to have contained 'inter alia' the whole of the Poenulus. We may fairly infer this from his express statement, 'aliquando in aliquot membranes pervetustas incidi in quibus scriptus erat Poenulus,' coupled with the number of citations he has made of T-readings throughout the play (vv. 19, 30, 47, 55, 137, 231, 245 ?, 319, 371, 413, 471, 478, 530, 580, 770, 778, 977, 1033, 1312).¹ The limited range of the dr.-variants (from Pseud. 730, through the Poenulus and Persa to Rud. 790) points rather to the collation of T having been the main factor in the marginalia and to readings from B having been adopted only here and there where the testimony of T was wanting. The only form in which any credence could be gained for the hypothesis that the dr.-variants include readings of B would thus reduce the part played by these B-readings to the very smallest. But, I fancy, most readers will agree that the hypothesis is impossible in any form. If the dr.-entries were taken from two sources, we should inevitably find the dr.-readings in pairs, at least in a sufficiently large number of cases to make their double origin evident.

On the other hand a moment's glance at the Oxford marginalia will show how readily a false theory of triple origin would be suggested. For they comprise (1) a large number of variants unmarked by any

¹ His citations for the Pseudolus begin with v. 738 and for the Rudens end either with v. 767 or with v. 724, in striking agreement with the range of the dr.-variants in these plays.

symbol (i.e. all the variants for the earlier plays and all for the plays after the Rudens, besides a large number in the remaining part), (2) a set marked poict., (3) a set marked dr. No one who had not accurate knowledge on the subject would imagine that readings followed by the symbol poict. in these four plays (Pseud., Poen., Pers., Rud.) came from one and the same source as the large number unprovided with any symbol in the earlier and later plays. If I am right in supposing the Paris Gryphius (of 1535) to be the transcript made by Passerat (but the handwriting cannot with certainty be ascribed to him) of the book lent him by Pithou, then this book must have been the Oxford Gryphius itself. Since the transcript was made in 1557, Pithou would be only eighteen years old at the time, and his ignorance of the composition of the volume which he lent would be very excusable. There are difficulties in this path of reasoning; but no difficulty so great as the difficulty of supposing the Oxford marginalia to be the collation of three, and not two, MSS.

This article is unduly long; but I should like to touch on one other point. We have seen that a great defect in the Duaren variants is the accidental mistakes made by Duaren in copying. Must we add to this that occasionally a reading has been deliberately set down not in the actual form in which it occurred in the MS., but in a corrected form, or (still worse) that an emendation suggested by the actual reading of the MS. has now and then been substituted for the reading itself? I should not like to affirm that there is absolutely no danger of this having happened; but everything points to such deliberate mistakes, if they did occur at all, having occurred very seldom. There is normally a careful distinction made between the actual reading of the MS. and the 'doctored' reading; e.g. ad Poen. 1355 AG. had uerbum quidem, app(arenter) haud verbum; ad Pseud. 738 hircum ab aliis dr. Is cum ab aliis poict. sed ab aliis un[sic] i, axillis; ad Poen. 501 profectos festos dr. | sed profestos legendum satis constat. And minutiae of spelling are carefully noted: e.g. ad Pseud. 814 alium dr. uno l; ad Pers. 659 redduco dup. d. I cannot find in these marginalia much support of Lambin's charge of carelessness against Turnèbe.1 The cases are very few

¹ What Turnèbe did to lay himself open to the charge of publishing as new emendations of his own those that had been already made by Camerarius or others before the first appearance of the Adversaria

where one is at a loss regarding the proper appreciation of a marginal variant.

W. M. LINDSAY.

I will not trespass far upon the space of the Classical Review; for I do not think it is possible at the present moment to say the last word about this curious and important literary problem. Every editor of Plautus will have to examine the evidence for himself and draw his own conclusions. But I will very briefly indicate the impression which Mr. Lindsay's statement has made upon my mind, and add a few comments on it, and a few new points which

claim consideration.

Mr. Lindsay seems to have stated his own position at length rather than to have answered my criticisms. I do not complain of this; but it has the disadvantage of leaving the argument very much where it was. Mr. Lindsay's order of proceeding seems, by his own admission, to have been a strange one. First he publishes these readings in the Berliner Philologische Wochenschrift as a collation of the Cod. Turn. without much discussion of evidence; then, in his published volume, he proceeds on the tacit assumption that no one questions their identity; finally, in reply to criticism, he offers evidence. not this 'sentence first, trial afterwards'! The difference between me and Mr. Lindsay is mainly this: I demand proof; he is content with an inference based upon a priori probability or possibility. He may be right; but I am inclined to think that before an inference of such importance to the future of Plautine criticism can be accepted, it must emerge from the region of the 'not proven,' and stand forth clear of all reasonable objection. Otherwise our critical apparatus will be complicated to no purpose, and our reconstructions of the stemma of MSS, will rest on no better foundation than the baseless fabric of a vision.

(1) The difficulty of Mr. Lindsay's position is well illustrated by the fact that in the list of readings which he has published in the *Philologus* (Supplem. vii. p. 117 ff.) he is often driven to put down two different readings to the Cod. Turn., with a query after one of them. Why, too, has he there mixed

in 1564, was probably in reality this. He made emendations of the current text with the help of T, and when the time came for publication of these emendations in his Adversaria, he omitted the duty of striking out all of them which had been later made independently by Camerarius with the help of B, a MS. very similar to T.

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(2) I have never denied that these marginalia are by the hand of Duaren, whoever he was (it does not much matter who wrote them), or that they are a copy of some original. On the contrary I entirely agree that they are.—So too with some other points on which Mr. Lindsay dwells at such length.

(3) Some of my main points are not touched on at all; e.g. that the entry ex fragmentis, etc. refers only to a single line. My figures remain unchallenged.

(4) 'The excellence of these new variants ...is happily beyond dispute.' All of them?

(5) The reading Punicust guggast homo (Poen. 977) is expressly attested by Lambinus as found 'in nostris libris'; and of the reading haud verbum quidem he says 'sic perspicue scriptum est in nostris libris veteribus.' It is in vain to argue, as Mr. Lindsay does, that these were not MSS. but printed editions with marginalia. It is impossible to get over the fact that Lamb., in his note on Cas. 414, enumerates seven MSS. as at his disposal in the unmistakable words 'sic habent omnes libri et mss. et impressi quos quidem viderim, septem mss., totidem impressi. Comparatively few indeed are the good readings of the Bodleian volume which were unknown before on MS. evidence. As to the symbol C (Canticum) in the heading of Pseud. IV. ii., it appears in all the Palatine MSS. in this very place!

(6) It is out of place to appeal to the authority of Prof. Goetz, whose statement of 1884 may possibly be modified, if he reads the evidence I have now given against it.

(7) The discoverer of the Ambrosian was not Cardinal Mai, but an obscure assistant in the Ambrosian Library in the eighteenth century (named Branca), unless indeed Turnèbe discovered and used it in the sixteenth century. Mr. Lindsay ought not to have waved my argument on this point aside by his ipse dixit. My distinction between schedae antiquae and liber vetus he wholly ignores. Yet it is to B (not to the Cod. Turn.) that I think the entry on the fly leaf ('ex veteri libro') refers. What

evidence is there that the home of the Cod. Turn. was France?

(8) My interpretation of alta manus is 'not to be taken seriously.' Let Mr. Lindsay look again at Ducange, under 'Scriptura,' and the authorities there quoted.

(9) My argument as to 'Adriani Tornebi' not being a natural way for T. to describe himself in his own copy of Plautus does not 'appeal' to his mind. Perhaps it will appeal to others.

(10) I am glad, however, to see that one of my arguments does possess weight in his eyes, though he does not agree with me. To discuss this point fully would carry me beyond the limits of my space.

(11) New points. (a) The entry on Pseud. 732, which ought on Mr. Lindsay's theory to have come not in the middle of the book but before the Bacchides, encloses the entry Caristo dr. The latter, then, may well have been an earlier entry than the first mention of Turnèbe's name. [Parenthetically, Mr. Lindsay's amended theory that dr. is a monogram representing a. tr. (Adr. Turnèbe), however ingenious, does not 'appeal' to me. Till the meaning of dr. is settled, I repeat, there will be no solution of the problem of this book.]

(b) Does the long entry on Rud. 776, beginning forte occentarit and containing a quotation from Festus (mutilated at the end) come from the Burney2? I do not gather so from Mr. Lindsay's p. 49. If not, it throws new light on my suggestion that these marginalia come in some cases, directly or indirectly, from Lambinus. The quotation is followed by the words peto hic Persa scena Satin Athenae, etc., in which the first person sing. (peto) arrests attention. Who is the 'I'? Turn to Lambinus' notes on this passage and on the scene of the Persa beginning Satin Athenae (l. 569, pp. 958 and 915 of his ed. of 1577) and there is the reading occentarit and the whole content of this entry reproduced. Is this a mere coincidence? It would be well worth while if Mr. Lindsay would report more fully on the marginalia of Lambinus' copy of Plautus in the Bibliothèque Nationale (Rés. p. Y. c. 235).

E. A. Sonnenschein.

THE NEW FRAGMENT OF JUVENAL.

A paraphrase will save more than its own bulk of notes :—

'Wherever a cinaedus is kept he taints the household. Folks let these fellows eat and drink with them, and merely have the vessels washed, not shivered to atoms as they should be, when such lips have touched them. So even the lanista's establishment is better ordered than yours; for he separates the vile from the decent (and similarly the retiarius' net is not kept along with his tunic, nor does he put his galerus in the same cupboard as his trident): in the training-school and even in the gaol such creatures herd apart: but your wife con-demns you to drink out of the same cup as these gentry, with whom the poorest trull would refuse to sip the choicest wine. Them do women consult about marriage and divorce,'-verse 18 I do not understand, so I must leave it out,- 'from them do they learn lascivious motions and whatever else the teacher knows. But beware: that teacher is not always what he seems: true, he darkens his eyes and dresses like a woman, but adultery is his design. Mistrust him the more for his show of effeminacy: he is a valiant mattress-knight: there Triphallus drops the mask of Thais.'—Now he addresses the cinaedus himself:—' Whom are you fooling? not me: play this farce to those who cannot pierce the masquerade. I wager you are every inch a man: do you own it? or must we wring the truth out of the maidservants?

1-3. Write and punctuate (Athenaeum, May 13)

in quacumque domo uiuit luditque professus obscenum, tremula promittit et omnia dextra, inuenies omnis turpes similesque cinaedis.

quacumque is relative. I have moved et, which the scribe transferred to its natural place because he knew no reason why he should not: he has done the same with qui at xiii 86. et in the third place does not seem to be found in Juvenal, but promittens would be rough and omnia is too apposite to lose: he is $\pi a \nu \tau \sigma \pi a \theta \dot{\eta}_{S}$, as the sequel will show. $tremula\ dextra$ is partly explained by 24.

6. colocyntha = σικύα = os quale est Sex. Clodii apud Ciceronem. The use of the σικύα in women's ailments is described by Hippocrates ed. Foes. p. 263 16 ταύτη χρη

σικύην ἐμποιήσαι ἐς τὴν μήτρην, 581 37 ἐνθέσθω ἐς τὸ αίδοῖον τὸ ἄκρον τῆς σικύης ὡς ἐσωτάτω, 680 43 ἡ γυνὴ ἀμφικαθεζέσθω περὶ τὴν βάλανον τῆς σικύης, τὸ αίδοῖον ποιήσασα ὁκοῖον δεῖ.

barbata chelidon = pudendum muliebre = os quale est Gellii apud Catullum. Suidas λέγεται χ ελιδ ὼ ν καὶ τῶν γυναικῶν τὸ μόριον, Priap. 12 13 sq. 'qui tanto patet indecens hiatu | barbato macer eminente naso.' Mr Platt refers me to Ar. Lys. 770–7 ὁπόταν πτήξωνι χελιδόνες...ἀπόσχωνταί τε φαλήτων... ἢν δὲ διαστῶσιν καὶ ἀναπτῶνται πτερύγεσω (see πτερύγωμα Pollux ii 174)...σαφής γ' ὁ

χρησμὸς νη Δί'.

8 sq. 'longe migrare iubetur | psillus ab eupholio.' The change is so slight and the verbal antithesis so perfect that I suspect this should be written 'psellus ab euphono' (euphono has occurred also to Mr Platt). It is conceivable that ψελλός, like βάταλος (Aeschin. Timarch. 131), signified ἄνανδρος: Plat. Gorg. p. 485 C ὅταν ἀνδρὸς ἀκούση τω ψελλιζομένου... φαίνεται... ἄνανδρον, Phaedr. append. Perott. 8 2 sq. 'fracte loquendo... famam cinaedi traxerat certissimam': and that the poet playfully uses εὖφωνος, the opposite of ψελλός, as the opposite of ἄνανδρος. The Greek adjectives are less surprising than the Greek participle chironomunta v 121.

11. pulsatum would be grammar, and either pulsantem (Postgate) or pulsarum would be sense into the bargain, but none of the three would have been altered to pulsatam (feminie): I conjecture therefore that arma is a metrical interpolation and that the verse

ran

munimenta umeri pertusa hastamque tridentem,

i.e. ptusa astamque: Val. Fl. i 641 'trifida Neptunus in hasta.' Thus galerum pertusum matches turpi tunicae: each noun has its disparaging epithet, and each epithet is also applicable to a cinaedus. The whole digression 9-12 is absurdly frivolous.

12 sq. Write and punctuate (Ath., May 13)

pars ultima ludi accipit has animas aliusque in carcere neruos.

'these beings are consigned to the lowest corner of the training-school and to a separate cell in the very prison.' The scribe mistook neruös for acc. plur., but see xiii 50 toruös P, iv 120 laeuo P, i.e. laeuom: in the best MSS of Martial these forms are

15. recusat MS, recuset Platt rightly: 'would refuse,' if the proposal were made, which is very unlikely, since Alban and Surrentine are not often offered to a 'lupa

ruinosi sepulchri.'

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May 13)

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26. This sham weakling is compared to a Triphallus who saltat Thaida, plays the part of Thais in a mime. Either Thais saltata Triphallo stands for Triphallus qui Thaida saltauit, as deuictam Asiam stands for deuictorem Asiae at Verg. Aen. xi 268; or else exuit personam Thais means Thais, quae persona est, exuitur (deponitur), as altera maternos exaequat turba Libones (Scipionibus paternis) means altera turba, materni Libones, exaequatur at Prop. iv 11 31.

27. Punctuate (Ath., May 13)

quem rides? aliis hunc mimum!

I suppose I need not illustrate the omission of the verb: Plaut. capt. 551 'ultro istum a me.'

A. E. HOUSMAN.

In whatever house there is a profligate, he will make all the rest like himself. Omnia seems right: ep. iii. 77 'omnia novit Graeculus esuriens.' xi. 174 'omnique libidinis arte.'

In line 6 Colocyntha and Chelidon seem to be names of cinaedi, feminine, like Catullus' Attis, on account of their feminine habits. The form κολόκυνθα is found in Dioscorides 2. 162 (κολόκυνθα ἐδώδιμος) and 4. 178 (κολόκυνθα αἰγός). Chelidon (the name of the mistress of Verres) appears as one of Cleopatra's eunuchs in Seneca ep. 87, 16 'Chelidon, unus ex Cleopatrae mollibus, patrimonium grande possedit.' The name Eupholio (line 9) is probably wrong. Perhaps it should be Euphronio: a Euphronius was the teacher of the children of Antony and Cleopatra, Plut. Ant. 72.

Line 17 should run accipit has animas aliosque in careere nervus (or nervos as nom.). 'Such creatures and others are visited with the stocks in prison': cp. viii. 254 'plebeiae Deciorum animae.' Lucan 5, 322 'imbelles

anımas

S. G. OWEN.

THE fragment of Juvenal discovered by Mr. Winstedt should probably be placed after vi. 345. It must have dropped out of the text of the archetype of most MSS. at an early stage. I think it genuine, and not an alternative draft. Line 346 (audio quid ueteres olim moneatis amici) in the ordinary text, seems to have been patched up to make sense of the curtailed text. These recovered 34 lines formed a page in a MS. which was lost, as a page seems to have been lost, containing scholia, at the beginning of sat. vi. in P (Friedländer Juv. i. 117). Lost pages in Juvenal MSS, contain generally about 30 lines on a page; thus of Hosius' MSS. Monac. 23475 omits vi. 269-302 (33 lines), viii. 1-63 (31+32 lines), x. 9-70 (35+35) lines; and Voss 18 omits xiii. 169-224 (27 + 28 lines). The presence or absence of scholia may account for the variations in the number of lines. Though the Bodleian MS. must descend from a MS. of early date, being anterior to the loss of this page, there seems no reason to doubt that the MS. itself represents generally the Cornutus recension of the w group of MSS. I suggest that lines 1-3 should run:

In quacumque domo uiuit luditque professus obscenum, et tremula promittit <ibi>omnia dextra,

inuenies omnis turpes similesque cinaedis.

6 It is obvious to conjecture that 'Colocyntha' and 'Chelidon' are the names of 'molles': and the guess is confirmed by Seneca *Epist. Mor.* xiii. 2 § 16 Chelidon, unus ex Cleopatrae mollibus.

11 I think that we should read 'pulsatum,' and take it a supine. My first thought was to translate: 'Nor does the gladiator who fights barefaced deposit his trident in the same closet to knock against the shoulder-pieces and the arms [of the mollis].' But Professor Ridgeway and Mr. Lendrum point out that it is possible to translate: 'Nor does the gladiator who fights barefaced deposit in the same closet his shoulder-pieces and his trident to knock against the arms [of the mollis],' and I gladly accept the correction.

H. J

26 May, 1899.

Mr. Winsted wishes me to state that in line 5 of the new passage the MS. reads 'permittunt,' as I had already printed from conjecture: and that in line 9 'et' is an erratum for 'ab.' Besides this there should be stops at the end of lines 3 and 5.

I am glad to find that the passage which I exhibited in the May issue 'properantifalce dolatum,' should already have received the finer touches from the hands of other English scholars. I should accept Messrs.

Housman and Owen's punctuation of the first three lines and the same scholars' has and neruus (or neruos) in 13 though with Housman's alius, and withdraw my interpretation of 6. I should think Mr. Platt's recuset in 15 little less than certain and Mr. Housman's punctuation of 26, 27 welldeserving of consideration. As the lastnamed scholar has referred to pulsantem which I conjectured in my note on 11 (where for quassatam read quassatum: the sense would be 'crazy,' 'battered'), I should like to say that, if I am right in this suggestion, I should regard the corruption as due to accident: and that the meaning of these lines is 'not even (nec) will retiarii associate with the molles among their number (turpi tunicae); and the same idea is carried on in the next line (nec cella ponit eadem, etc., i.e. as the molles); and so Mr. Duff understands the passage. In conclusion I would refer to the biting jests of Claudian on the eunuch Eutropius in Eutrop. i. 359 sqq. which lend some support to omnia and illustrate dextra (2 and 24).

'nil negat et sese uel non poscentibus offert (364)—quidquid amas dabit illa manus. communiter omni | fungitur officio gaudetque potentia flecti—accipit et trabeas argutae praemia dextrae.'

J. P. P.

POSTSCRIPT.

Mr. Duff sends me the following:-

'I believe that *pulsatam* is corrupt, and has taken the place of some adjective agreeing with *arma*, which is in apposition with *tridentem: arma* is used often enough of offensive weapons. Such an adjective as *Sempronia* would give the sense of what I think Juvenal may have written.

It seems that in 1. 28 (purum—pergula), the husband is apostrophising the cinaedus. The apostrophe is abrupt, but hardly more so than others in the satire: cf. ll. 214, 219.

I take omnis to be feminine, and the unexpressed subject of permittunt to be mulieres.'

J. D. D.

MÜNZER AND KALKMANN ON PLINY'S SOURCES.

Beiträge zur Quellenkritik der Naturgeschichte des Plinius, von F. Münzer. Berlin, 1897, pp. xii. and 432. 12m. Die Quellen der Kunstgeschichte des Plinius, von A. Kalkmann. Berlin, 1898, pp.

viii. and 260. 9m.

Dr. MÜNZER has here undertaken a very useful piece of work. While Pliny's Naturalis Historia has always been a storehouse of matter for the writer on any branch of Roman history or civilisation, the multifarious facts found in any compilation of the kind must obviously depend for their value rather on the weight of the authorities from whom they were originally drawn than on the writer to whom we immediately owe them. It is not possible to consider Pliny's statements all of equally great or small value as coming from Pliny, while it is frequently possible by close examination to trace them back to the original fount, or to the intermediary to whom Pliny owes them, even in many cases in which he does not name his authority. Dr. Münzer is already known as a student of Pliny's sources by his essay, Zur Kunstgeschichte des Plinius (Hermes, xxx. 1895); this special enquiry

he has now followed up by an examination into Pliny's methods of work and use of his authorities throughout. Beginning simply with the desire to examine the sources for statements referring to Roman republican times, and so help to determine their value, he has been led to extend his work by realizing the tendency of any such enquiry to become one-sided when not based on a scrutiny of the general system on which Pliny can be seen to have worked. Such a scrutiny he has carried out in the most thoroughgoing way, arguing partly from comparison of different passages in the Naturalis Historia one with another, partly from comparison of Pliny either with extant works and fragments of authors cited by him, or with other writers who can be shown to have drawn from the same sources. Incidentally he gives from a wider outlook some good criticisms on different smaller treatises on Pliny's authorities for particular books. Pliny has often been accused of mere unintelligent copying of passages from other authors to make a patchwork of his own, but Dr. Münzer, while enquiring into his way of comparing and arranging his facts, has to a considerable extent vindicated

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his author in the same way that Freeman did Diodoros. In the positive results which he tries to establish Münzer shows as a rule much moderation, though occasionally his learned enthusiasm carries him away, and he is led to build up a personality out of too slight materials, as in the pages (167-172) which deal with Annius Fetialis, an author once quoted by Pliny in the body of his book. It is difficult in a limited space to do justice to the mass of small cumulative proofs on which Münzer bases his conclusions, but a few general results may be given. On the whole he brings into strong relief the part played by Varro among Pliny's direct authorities, who were as a rule the writers of the generations immediately preceding him, the contemporaries of Julius Caesar and Augustus. The name of Varro meets us at every turn in the pages of Pliny, and the younger writer's debt to the older proves even greater than he acknowledges it to be. In some cases Pliny's Greek or early Roman authorities were only known to him through Varro; in other cases he combined this indirect use with occasional direct reference to an earlier writer: e.g., both direct and indirect use of Herodotus can be shown (pp. 17, 419). Of the indirect use of the elder historians xxxvi. 107 is a good instance. Pliny gives an account of the punishment of suicides which is found again in Servius, Aen. xii. 603, where Cassius Hemina is quoted as the authority, and Varro named immediately afterwards. Pliny does not even name Hemina among the authors for xxxvi. and Münzer (p. 184) argues with great probability that he here drew on Hemina through Varro. He is less convincing when he proceeds to infer the same indirect use of Hemina for xiii. 84, where Pliny is quoting him to confute Varro. A large number of instances are examined where analogies between Pliny and some other later writer can be explained better by the existence of an older common authority for both than by assuming that one drew upon the other. Thus Valerius Maximus 8, 13, ext. 1—7, gives the same instances of longevity as Pliny vii. 154ff, but with numerous small differences, which can best be explained by supposing the use of a second source by Pliny, who moreover, in vii. 85, quotes Varro as his authority for facts found also in Valerius Maximus 1, 8, ext. 14. Again, where a group of facts drawn from different authors occurs in very much the same connexion in Pliny and other writers, it is natural to suppose that the quotations in each case have come through the

same channel. In pp. 189 ff. Münzer traces a good instance of this in a quotation, xiv. 89, from Fabius Pictor; and so in many other cases. Many questions are doubtless still left open to discussion, and some weak points may be noted. In xxxiv. 7 Münzer can hardly be right (p. 124) in basing an argument on Pliny's placing the fall of Corinth in Ol. 156, when the reading of Bamb., cluili, so clearly points to Ol. 158 as the date really given by Pliny. On p. 175 the passage from Columella brought forward doubtless shows that Cicero is the authority for vii. 75, but Münzer is over subtle in trying to prove that Pliny drew on Cicero through Varro. Again, Columella vii. 1, 3 would seem to make against rather than for the supposition (p. 356) that Pliny's very similar statement (viii. 206) is drawn from Nigidius. In so wide an enquiry it is natural that some parts should be less fully worked out than others, for example the chapters on Juba and the value of facts for which Juba is the channel, and on Nepos, whose position as a source for facts of Roman history is important. In conclusion Dr. Münzer gives an index to the passages of Pliny quoted in the book. A second index to the other authors would be

a helpful addition.

Dr. A. Kalkmann in his present treatise has confined himself to a study of Pliny's ultimate sources for the history of art. In doing this, he is necessarily dealing with matter which has been already worked over by other scholars, but he has much that is new and suggestive of his own to add. The most prominent feature is an effort to show (pp. 1-64), that the chronological tables of Apollodoros were in the hands of Pliny, who drew upon them largely. While recognising Xenokrates as the writer to whom the kernel of art criticism in books xxxiv. and xxxv. is due, Dr. Kalkmann believes that much of what has recently been assigned to him or to Antigonos, notably the chronology of bronze workers (xxxiv. 49 ff.) and painters, should be attributed rather to Apollodoros. He supports this view by a number of arguments. The chronology of Apollodoros ended with Ol. 159, after which year Pliny gives no dates, his latest (xxxiv. 52), being Ol. 156. The occurrence in Pliny's chronological list of names of artists who are not discussed later on, is to be explained by the use of an independent authority for dates, as are also other difficulties, in particular Pliny's discussion of the dates of the early painters (xxxv. 54-58). Here Pliny confutes his

first authority, a Greek chronicler, presumably Apollodoros, who gave Ol. 90 as the first date in the history of painting, by facts drawn from another source, probably Varro based on Xenokrates, where an account of the earliest artists quorum aetas non traditur, was given without dates. Kalkmann sets forth very fully these and other reasons for thinking that Pliny's dates are drawn directly from Apollodoros, but fails, I think, to shake the one argument that seems conclusive against his theory. The dates given by Pliny assume a gap between Ol. 121, and the renascence ascribed to Ol. 156, and, except in the case of Pyromachos, they do not include the Pergamene school at all. This omission and the date at which the break in the history of art begins, accord perfectly well with the theory that Xenokrates and Antigonos are responsible for the earlier chronology, and Kalkmann assumes that Apollodoros has simply followed them. The neglect of the Pergaartists, however, remains alike inexplicable and impossible, as Robert has well shown, in the case of a writer whose residence in the country must have given him every opportunity of becoming acquainted with their style, and who dedicated his own work to the kings of Pergamon.

In his later chapters, Kalkmann discusses the part that may, with a fair degree of probability, be assigned to Pliny's other authorities. He follows other writers in ascribing to Mucianus a large part of the history of marble scuptors, and displays

much ingenuity in hunting for further traces of him in the other books, e.g. the statement (xxxv. 50), as to the four colours used by the earlier artists. To rhetorical sources he would refer some of the scattered artistic criticisms, particularly such as have an ethical tinge about them like that on Nikophanes in xxxv. 111. In the very discrepant judgment passed on the same artist in xxxv. 137, the phrase soli artifices points, as Münzer believes, to Xenokrates and At p. 184 ff. is given a good Antigonos. analysis of the alphabetical lists, principally useful as showing at a glance the element drawn from Roman catalogues. In xxxv. 108 ff. a new genealogy of the painters is suggested. In § 108, Kalkmann (pp. 57 ff.) would read: Nicomachus, Aristonis filius (Voss. ariste; Ricc. aristicheimi; Bamb. aristiaci). This Ariston he believes to be identical with the one mentioned in § 111, and son of the elder Aristeides, while in § 110 he reads (Nicomachus) discipulos habuit Aristiden fratrem et Aristonem filium. assuming two artists called Ariston, he makes the two contemporaries of Apelles, Nicomachos, and the younger Aristeides, belong to the same generation as grandsons of the elder Aristeides. It is impossible to follow Dr. Kalkmann in many of the conclusions that he draws; at the same time, his candour and thorough methods of research are apparent throughout, and the book must be of interest to all students of the sources of Pliny.

K. JEX-BLAKE.

BALY'S EUR-ARYAN ROOTS.

Eur-Aryan Roots with their English Derivatives and the Corresponding Words in the Cognate Languages, compared and systematically arranged. By J. Ball, M.A., Worcester College, Oxon., Fellow of Calcutta University, sometime Archdeacon of Calcutta. Vol. i. pp. xxviii., 781. Royal 8vo. London: Kegan Paul, Trench, Trübner & Co., Ltd. 1897. 50s. net.

This large and handsome volume is the first part of a work intended, as the author tells us in his Preface, 'to present to English readers in as popular a form as the subject admits, and with an especial reference to the English language, the results recently obtained by German philologists.' It contains, in fact, a careful summary of the

results of research as given in a large number of works, of very unequal value, which are cited in the Preface. The author has not apparently grappled with more recent work contained in the numerous philological journals. This is a pity, because a certain amount of his material is somewhat antiquated. The book has also had the misfortune to appear contemporaneously with the new edition of Brugmann's Grundriss, vol. i., which could not therefore be utilised in its preparation. The frequent references to Brugmann thus represent not that scholar's most recent deliverances on questions of phonetics, but those views, sometimes widely divergent, which he held eleven years before.

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author applies to the group of languages with which he deals is a strange formation and certainly does not bear its meaning on its face. It is undoubtedly true, as the author asserts, that Indo-Germanic and Indo-European are cumbrous terms; it is not so clear however that Eur-Aryan is, as the author claims it is, more exact than either. Indeed, if a shorter word for the group of languages be wanted, Eur-Aryan cannot compare favourably with the late Mr. Wharton's Celtindic.

It may also be doubted whether the author has not carried the reduction of all forms to original roots somewhat farther than existing evidence warrants. There are a considerable number of noun-forms which are clearly descended from a period before the separation of the languages, though no exists with which they have any strong claim to be connected. The reduction of such forms to roots, or rather the placing of such forms under a general root with which they have no obvious connexion of meaning, is a practice taken over with doubtful advantage from the grammarians The author, however, goes sometimes still further. It is possible, for example, that the Greek ήδύς, the Latin suāvis, come from a pronominal element with a suffixed element and that the meaning of the word corresponding to ηδομαι in the original language was 'makes one's own,' 'suits one's self' (p. 526). But it is impossible with our present knowledge to prove this, and, in any case, our present knowledge certainly does not warrant our placing such forms under the root dhē. For most purposes it would have been more convenient and more accurate to keep such words, as Fick does, in the stem form and to have assigned the adjective suādú-s to the original tongue. Further analysis of the root suad- can be merely speculation, the truth or falsity of which cannot be tested.

Subject to this limitation the work will be found useful, particularly by those who wish to find all the important cognates of any English word under one head. The method of the author will be best illustrated by one example.

Eur-Ar.: \(\sqrt{UER}, a man, hero, husband. \)
Sanscrit: vîra, a man, hero, vîryu, strong, nanly.

Zend: vīra, a man, vīrya, manly.

Greek: $\hat{\eta}_P$ for $F\eta_P$ in $\hat{\eta}_P\omega_S$, a hero (given after Curtius but marked in a note as not established).

Latin, vir- in vir, a man, virilis, manly, strong, — itas, manliness, virtus, courage,

virtue, virago, a masculine woman, decemviri, a committee of ten, trium-viri, of three.

This is followed by the Low Latin and Romance forms under which is explained amongst other words loup-garou. Then come the Balto-Slavonic, the Teutonic and the Celtic forms.

A separate heading is formed in each case by the English Derivatives. In this instance these include:—

Greek: hero, heroine, -ism. heroic.

Latin: virile -ity, virago, decemvirate, triumvirate.

L. Latin and Romance: virtue, vertu (taste), virtuoso, virtuous.

Teutonic: werewolf, wergild, world, ly,

It might be objected that, though it is desirable to have the Zend forms, they are not entitled to a separate section for themselves if the Baltic forms are not separated from the Slavonic. It might also be urged that the Romance forms are not quite on the same level historically as the other sections. But these points are of little importance. A more serious matter is a certain looseness with regard to important phonetic principles. In the instance just cited, what evidence is there to warrant the assumption that the root is yer and not wir? The author himself considers that ηρως does not really belong to this root. The e of the Irish fer and of the Teutonic forms can be shown by parallels to be a change within those languages of an original i. The root or, as I should prefer, the stem, in this case should therefore have been given as uir-o, or if the author had chosen he could have followed Uhlenbeck and others in connecting the form with the root uī- of Latin vi-s, &c.

The statements about vowel-gradation also are not always so precise as could be desired. For instance, in the example (p. ix.) given as typical—the root tel-, tll-—, it is impossible to explain forms like the Sanskrit tola, which in the o contains a u-diphthong, without assuming that the influence of analogy, starting from forms in tul-, has carried over the root from one series of gradations to another. Similarly there is no authority for an original \bar{c} - as a parallel form to al- (p. 19) even if the equation of Skt. ek-a- with Lat. aequ-o-s be admitted.

Phonetic laws are discarded without explanation in such derivations as δφις from an original form with βh, Lat. annue, connected with the root of Féros 'at-standing for an older vat' (p. 3 n.), an explanation which must be erroneously attributed to Brugmann. So also whatever

the origin of ponti-jex (p. 527 and elsewhere) it cannot be phonetically connected with the original Latin numeral for 5; the equation 'Lat. pompi-: ponti-:= $\pi \epsilon \mu \pi \epsilon$: $\pi \epsilon \nu \tau \epsilon$ ' is impossible, and the name Pontius is not Latin at all but Oscan as, apart from other considerations, its occurrences in history prove.

Statements of this sort, which are somewhat numerous, must cause the connexions which are made between various series of words to be received with caution. There is, however, a vast amount of useful learning in the work, and there are many valuable contributions to the history of the meaning of English words and proper names which are illustrated with an historical and antiquarian knowledge too often conspicuously lacking in works of this sort.

P. GILES.

NOTES.

BACCHYLIDES, Io (xix. Ken., xviii. Bl.), 33, 34;

ή ρά καὶ [___ __ _ ἄσπετοι μερίμν [_

Jebb supplies ℓs $\tau \ell \lambda o s$ $\sigma \phi$ $\ell \tau \epsilon \iota \rho o v$, Jurenka $a l \nu a$ $\gamma v l$ $\ell \lambda v \sigma a v$, in 33, and both read $\mu \ell \rho \iota \mu \nu a$ in 34. Professor Jebb's $\sigma \phi$ $\ell \tau \epsilon \iota \rho o v$ seems exactly what is wanted; but, as the papyrus marks with an accent the second syllable of $\mu \epsilon \rho \iota \mu v$, it seems hazardous, in the case of a supplement which must be more or less conjectural, to neglect this indication. I propose to read

ή ρα καὶ εὐφρόναι σφ' ἔτειρον ἄσπετοι μερίμναις;

Compare Od. xv. 392 αίδε δὲ νύκτες ἀθέσφατοι, and xi. 373 νὸξ δ΄ ἢδε μάλα μακρὴ ἀθέσφατος. For the plural of εὐφρόνη cp. e.g. Hdtus, vi. 56. The meaning is: Did he fall asleep, worn out by long night watches? For the transformed Homeric phrase, compare Pindar's θάλλοντος συμποσίου (Isth. v. 1), suggested by δαίτα θάλειαν.

J. B. BURY.

A CURIOUS PARALLEL.

λάϊνον έσσο χιτώνα. Π. Γ. 57.

In a lately published novel, 'Two men of Mendip' by W. Raymond, 1898, one of the characters says:—
'If ever day should come, an' no welcome in Charterhouse, let John Winterhead put on the green waistcoat, by which he meant lie under the graveyard sod.'

Is this (as seems implied) a genuine Somersetshire country phrase? If so, it is a remarkable parallel to the Homeric expression, and to the Aeschylean χθονδε τρίμοιρον χλαϊναν λαβών.—Ag. 870.

W. C. GREEN.

AESCHYLUS, Agam. 1266.—The MSS. read $i\tau'$ ές $\phi\theta\delta\rho\rho\nu$ ΠΕ CONTAΓΑΘ Ω Δ ἀμείβομαι (corr. to ἀμείψομαι), for which editors give

ἴτ' ἐε φθόρον πεσόντ'· ἐγὰ δ' ἄμ' ἔψομαι (Hermann), or

1τ' ès φθόρον πεσόντα θ' &δ' ἀμείβομαι (Verrall). Neither of these emendations is satisfactory. Apart from minor objections, every one must feel that πεσόντα is painfully weak and otiose. I suggest

ἴτ' ἐς φθόρον· κάτω γὰρ ὧδ' ἀμείβομαι
'Off, cursed things! even thus (without adornment)
I pass to the world below.'

REYNOLD A. NICHOLSON.

On $o\bar{l}\sigma\theta$ ' b $\delta\rho\hat{a}\sigma o\nu$.—A curious parallel to this well-known construction occurs in the Dīvān of Hāfiz (Ed. Rosenzweig, iii. 204, 5):

man nagüyam kih kanûn bā kih nishîn û chih binûsh.

'I will not say with whom now you are to sit and what you are to drink' (literally, in Greek: ἐγὼ οἰκ ἐρῶ ὅτι νῦν παρ' ῷ κάθισον καὶ ỡ πίε). The parallel is incomplete in so far as Persian makes no formal distinction between the relative and interrogative pronouns, but the imperatives—and this is the main point—are clearly subordinate. Dr. Postgate, who has examined the subordinated constructions of the Greek imperative in a very interesting paper (Transactions of the Cambridge Philological Society, vol. iii. p. 53 seqq.) quotes analogous usages in Old and Middle High German. In Persian the construction is exceedingly rare: it seems not to have been noticed by grammarians, and I am unable at present to supply a second example of it.

REYNOLD A. NICHOLSON.

PLAUTUS, 'PSEUDOLUS,' I. 3. 117 (l. 351).

'Quid ais, quantum terra < m > tetigit hominum periurissime.'

Ir has been established from the Ambrosian Palimpsest, by Geppert (followed by Studenund, Ussing, Goetz, etc.), that tetigit is the true reading in this passage, in place of tegit of the later MSS. As re-

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tion may ost before a gloss,

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51). um per-

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ian Pal-Ussing, in this As regards the meaning of the expression terram tangere, Ussing says that it means nasci, and, as far as we can gather from his note, Geppert seems to take the same view.

But is there not a reference to the custom of lay ing the hand upon the earth in solemn oaths and adjurations, to avert the displeasure of the nether gods? Cf. Il. ix. 567:

πολλά δὲ καὶ γαῖαν πολυφόρβην χερσίν άλοία κικλήσκουσ' 'Αΐδην κ.τ.λ.

Also Bacchylides viii. 3:

γὰ δ'ἐπισκήπτων χέρα, κομπάσομαι. And with xépa omitted, v. 43.

γὰ δ'ἐπισκήπτων πιφαύσκω.

c.f. also Varro on Macrob. Sat. iii. 9, 12. : Tellus mater, teque Iuppiter obtestor. Cum Tellurem dicit manibus terram tangit; cum Iovem dicit manus ad caelum tollit.

And also Macrob. Sat. i. 10, 21:

Huic deae sedentes vota concipiunt, terramque de industria tanqunt.

We might also compare Cic. De Har. Resp. xi. 23 (where the MSS. rightly read terram).

The expression also occurs elsewhere in Plautus, Most. 471.

Aedes ne attigatis. tangite vos quoque terram.

Thus we see that touching the ground was a common formality in taking oaths; and as in this passage of the Pseudolus we hear that Ballio had taken a formal oath-conceptis verbis-it is not unlikely that we have a reference to this custom in the expression terram tangere, which would thus somewhat correspond to our expression, 'kissing the Book.'
The passage might then be translated:

Cal.—'You, I say, most perjured knave that ever kissed the Book! did you not swear that you would sell the girl to no one but me?'

Ball.—'I confess it.'
Cal.—'And in set form?' (conceptis verbis).
Ball.—'Ay, and subtle likewise.'

Lad that Ussing does not at

It may be remarked that Ussing does not attempt to give any authority for terram tangers = nasei; while A. Palmer, in order to avoid this interpretation, preferred to read terra tetulit.

F. A. Longworth.

On Horace, 'Satires,' II. 2, 89-93.—In the second Satire, 'an essay on the advantages of simple fare,' we have four lines which so perplexed Prof. Palmer that he suspected that they were spurious.

Rancidum aprum antiqui laudabant, non quia nasus Illis nullus erat; sed credo hac mente, quod hospes Tardius adveniens vitiatum commodius quam Integrum edax dominus consumeret. Hos utinam inter

Heroas natum tellus me prima tulisset!

I suggest the following paraphrase: 'Even putrid pork men would eat contentedly in old days and say it was very good, not that they had no sense of smell though, practically, I dare say, they had not so nice a sense as we have), but I suppose they felt that if a stranger came to a house some time after the pigkilling he could eat it with greater propriety and decency, though it was tainted, than the master of the house could when it was fresh, for that would have meant gluttony (edax). I wish I had lived in those days, when none over-ate himself, none refused fare however coarsely putrid.

We may note in favour of this :-

(1) laudabant: we must not make this mean that primitive people preferred pork high. The wish at the end proves that Horace is attributing some virtue of simple taste to them. And we may remember the use of laudo and ἐπαινῶ in the sense of 'I decline with thanks.' (2) rancidum is not to be minimized with thanks,' (2) rancidum is not to be minimized here as Comm. Cruq. suggests. The stronger its sense, the greater the virtue of the ancients. Hence the sense is not widely different from the passage Palmer sense is now wicely different from the passage Yalmer quotes, Lucr. vi. 1155. (3) erad may very well stand. Horace does not deny that the ancients had noses, but 'gives a possible hypothesis' certainly—that they had no nice sense of smell.\(^1\) (4) tardius: there is no question of 'arriving late for dinner'; and the whole point of the passage is that 'the household' had been obliged to 'leave the boar untouched so long'—not 'on the chance of a guest arriving' but long'—not 'on the chance of a guest arriving,' but because they could only consume it at once by gluttonous over-eating. A pig-killing would be an event to send word of to all the neighbours, who would drop in for some days after. (5) commodius seems most apt for the sense 'more harmoniously with the fitness of things and the rules of abstemious eating.'

In conclusion, no difficulty would have been raised had Horace said simply 'I suppose they felt that it was fitter to eat it tainted than to over-eat in order to finish it when fresh.' His playful opposition of the hospes and dominus seems to have misled.

T. NICKLIN.

ON VERGIL 'AEN.' VIII. 359 .- In the Classical Review for 1894, p. 300, discussing Verg. Aen. 5, 359, 'et clipeum efferri iussit, Didymaonis artes,

Neptuni sacro Danais de poste refixum,

hoc iuvenem egregium praestanti munere donat,' I suggested that the shield was a consecrated shield taken down by the Greeks from one of their own temples, and given, on account of its supposed sanctity, to some chosen champion, from whom it had been won in battle by Aeneas. Such a shield would probably be of great beauty, while its history would give it special interest as a trophy, so that it would fitly form a 'surpassing reward' for a 'peerless woulth'.

less youth. I quoted several instances of consecrated arms being taken from temples for use in emergencies (to which add Eur. Heracl. 695), but could find no instance of deliberate selection of them. Mr. A. G. Peskett, however, referred me to Tac. Ann. 15, 53, where it is said that the conspirator Scaevinus, with a view to the assessination of Nero, specially with a view to the assassination of Nero, specially wore a dagger which he had taken from the temple of Salus in Etruria velut magno operi sacrum, and I lately came by accident across a passage of Arrian which is still more important. It relates (Anabasis , 6) how, in the attack on a town of the Malli in India, Alexander himself seized a scaling-ladder and India, Alexander himself seized a scaling-ladder and mounted to the assault, followed by Peucestas δ τὴν lepàn ἀσπίδα φέρων, ἡν ἐκ τοῦ νεὼ τῆς ᾿Αθῆνῶς τῆς Ἰλιάδος λαβὼν ἀμὰ οἱ εἰχεν ᾿Αλέξανδρος καὶ πρὸ αὐτοῦ ἐφέρετο ἐν ταῖς μάχαις. This quotation seems to me decisive and to remove this passage, about which much rubbish has been written, from the still considerable light of Virgilian purgles. siderable list of Virgilian puzzles. T. E. PAGE.

¹ The Editor, doubting the validity of this exege Ine Editor, doubting the valuity of this exegesis, kindly points out however that Horace may well have used the same syntax as Propertius, and he quotes Prop. i. 11, 17. But Martial, i. 41, 18, writes non cuicunque datum est habere nasum, i.e. satirical wit; is it incredible that nasus might also mean not sense, but nicety of smell?

CORRESPONDENCE.

THE TOLEDO MS. OF THE AGRICOLA.

ALL students will be disappointed to hear that there appears to be no present probability of our obtaining any collation of this MS. As long ago as Jan. 26 I received information from Dr. R. Wuensch, of Breslau, respecting his visit to Toledo in 1896. It was only with difficulty that he obtained permission from the Bishop even to look at the MS., and no examination of its readings was allowed, on the ground that any publication of its contents would lessen its value. He was only able to note as follows. It is entitled 'Codex bibliothecae capitularis Toletanae No. 49, 2; chart. s. xv. fol. min., about 200 pages of 29 lines: fol. 1-15 contain the Germania, beginning, 'Cor. Taciti de vita moribus et origine Germanorum opus elegantissimum feliciter incipit,' and ending 'τέλως. Fulginie scriptum gerente me magistratum pu. scribae, Kal. Jun. 1471.' The Agricola then follows on fol. 16-36. Afterwards comes Joannis Antoni Campani oratoris oratio,' with the subscriptio: 'scripta per me M. Angelum Crullum (Trullum ?) Tudertinum Fulginii pu. scribam non Decembr. 1471.' Then follow letters of Plin. mi. (Books 8 and 9) with occasional signature of M. Angel Tuders, 1468. The MS. ends with an incomplete letter.

I should have long since communicated to the Review this information so kindly sent by Dr. Wuensch, but have been hoping from time to time to hear that the efforts made by Professor Gudeman to obtain a collation had been more successful. As no news to that effect has arrived, I fear we must conclude that his friend has also been stopped by an episcopal 'non possumus.' May 18, 1899.

H. FURNEAUX.

THE REVISED LATIN PRIMER.

THE announcement of a new revision of the Latin Primer raised many hopes: its publication has dashed them to the ground.

Some improvements have been made, but they are few. The book still remains thoroughly unsatisfactory. How unsatisfactory only those who have had to teach from it for years can adequately realise, and it would take many pages to show with any completeness; but the following are a few samples of its quality.

The last edition gives, what was much needed, lists of exceptions to the regular formation of the Gen. Plur. of the 3rd Decl. (Appendix IV. p. 225). The heading runs thus: 'Nouns not increasing in Genitive Singular which have Genitive Plural in -um.' This is the first and only intimation in the book that there is any connexion between increase in Gen. Sing. and the formation of Gen. Plur. Then there follows under 'I Stems '-

'In the Plural Genitive vates (bard) does vatum give: And generally agree with this panis, apis, volucris.

Now leaving out of consideration the late forms caedum, cladum, subolum, Cicero uses mensum, sedum, and vatium, Ovid ambagum there is no known instance of volucrium as a substantive; the only authority for panum is a grammarian, and apium is decidedly the better prose form.

Therefore each of the four statements is inaccurate in varying degree, while three words are omitted altogether.

The lists of 'Nouns increasing in Genitive Singular, which have Genitive Plural in -ium' are not more successful. Here under 'Consonant Stems '.

'ium in Plural Genitive os (ossis) and as (assis) give: mas, mus, dos and cos and lis, nox and nix, and sol and glis.

To class as, os and nox under Consonant Stems is, to say the least, doubtful doctrine. Cos and sol are usually believed to have no Gen. Plur. in use: the authority for dotum is as good as for panum, while no mention is made of fraus, laus, lar, strix, vis, fauces, renes, penates, optimates, the 'civitas' class, nor of the patrials in -as or -is (e.g. Arpinas, Quiris). To sum up, the lines omit more than they insert, and of what they do insert more than half is incorrect or doubtful.

The second list under this heading runs thus: 'I Stems,'

'In the Plural Genitive

frons (frontis) does frontium give : so frons (frondis), stirps, arx, and dens, mons, urbs, ars, bidens, and parens.'

This is an even more curious production than that which precedes. There appear frons (frontis) and mons but not fons or pons; frons (frondis) but not glans; dens but not gens or mens; ars but not pars; parens is bereft of infans, and such words as sors and cohors are ignored altogether.

Of course no sensible master would dream of letting his boys learn such erroneous and misleading lines. Their only use could be, like the drunken Helot, to serve as a warning of what a Grammar ought not to be; but it raises a blush to think that this is published urbi et orbi, under the special patronage of the great Head Masters of England, as the ripest fruit of our academic scholarship, matured by the experience and revisions of thirty-three years. Let us only hope that few foreigners see it.

In such fashion have the latest additions to the Primer been compiled. The revisers have not been more successful in dealing with the mistakes and omissions of the

earlier edition.

Mare still remains declined in full as a type of its class in spite of its Gen. Plur. marum.

The Future Imperative, after long banishment from other conjugations, still keeps its unintelligible footing in edo, § 140. Including this erratic survival there still are found four different methods of printing the Imperative Mood, a large discretion having, it would seem, been left to the printer's devil.

Tulisse still figures as derived from the Supine-stem of fero, amid a confused mass of Infinitives and Participles, Active and Passive, without order, name or meaning, inops inhumataque turba.

There still stands the brazen assertion in § 55 that acus, tribus, lacus, partus, portus, artus, 'have always -ubus.'

In defiance of Vergil glacies is still classed as 'singular only,' and veru, without reserve,

as 'dat. abl. plur. ubus.'

The rules for Gender still gape with omissions in well-nigh every part. The few lines given to the important subject of defective comparison of adjectives remain as before erroneous, misleading, and miserably inadequate.

Even in the 'Shorter' Primer the beginner cannot get beyond the 1st decl. without being taught that terrae is the ordinary Latin for 'on the ground.' myself to a few points out of many in the Accidence. Some consider the Syntax even more faulty.

In the above remarks I have confined

The comparison in this case would be particularly odious, and it is perhaps well

that limits of space forbid it.

If the Primer stood upon its own merits, it would not be worth criticising and would long since have given way to a better book even under the present discouragements at least one such has appeared-but, as it is, nearly all the great Head Masters use it and the small Head Masters must perforce follow suit, so that practically the whole Latin education of the country is based upon a work for which 'unsatisfactory' is a euphemism.

What harm has been and is being done to British scholarship it would be hard to estimate. Many of those who really have to teach the book have long been exasperated or depressed by the inferiority of the tool with which they are condemned to work. Not a few, it is to be feared, have never recovered the effects of being educated upon it themselves.

Of those who have, some in their bitterness of heart affirm the 'great' Head Masters to be like careless gods-'For they lie beside their nectar, and the

bolts are hurled

Far below them in the valleys,'

and it must be admitted that to be bombarded by bad grammar in a foreign tongue from a coign of vantage is trying.

Such, however, is not quite the writer's view. He believes those exalted personages to be not so much indifferent as simply ignorant of the facts of the case. Great Head Masters do not teach the Latin Primer to little boys. Their duties are multifarious and concerned with higher things. Naturally, therefore, they do not know the details of the book; and if perchance a voice of lamentation should now and then reach their ears, trusting to the author's honoured name, but forgetting that as a great Head Master himself he was little fitted by practical experience to write a working grammar for junior boys, they deem the cry 'like a tale of little meaning, though the words are strong.

Monopolies and vested interests are no doubt hard to deal with. But surely the matter is one of such importance, both for the credit of the scholastic profession and the future of British education, that those who are responsible for these difficulties ought to grapple with them promptly, and,

let us hope, effectually.

A. SLOMAN.

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ARCHAEOLOGY.

FOUGÈRES' MANTINÉE ET L'AR-CADIE ORIENTALE.

Mantinée et l'Arcadie Orientale, par Gustave Fougères, ancien membre de l'École Française d'Athènes, chargé du cours d'Archéologie et d'Histoire de l'Art à l'Université de Lille. Paris, Fontemoing, 1898. 8vo, pp. xvi, and 623, with 10 plates. 20 fr.

In the centre of the northern half of the eastern (and higher) Arcadian plain, more than 2,000 feet above the sea, is the site of the ancient Mantinea, the scene of excavations conducted by MM. Fougères and Bérard on behalf of the French School at Athens in 1887-9. Some chapters of this monograph are based directly upon those excavations, while the remainder contain a résumé of the history, constitution and customs of Mantinea itself, and the topography of the immediately surrounding country. It is rare that such elaborate study has been devoted to a town and territory of dimensions so modest; but even second-rate Greek states possess an individuality and a separate history of their own, and Mantinea is one of the most interesting of all second-rate Greek states. Partly from her strong democratic tendencies, and partly from her neighbourly quarrels with Tegea on such matters as the control of the water-system of the plain, she was generally anti-Spartan; Tegea, unable (as Fougères puts it) to exist with an enemy on either hand, was commonly on the Spartan side. But political combinations in the Peloponnese shift like the figures in a kaleidoscope; and Fougères, who has a natural partiality for Mantinea as the principal scene of his own labours, claims for her, not without some justice, that she was, in all her vicissitudes, on the side of liberty as against the successive attempts at domination of the Peloponnese made by Sparta, Thebes, Macedon, Sparta again (Agis and Cleomenes), and the Achaean league; while she had, for herself, neither the will nor the power to dominate at any time.

It will only be possible in this short notice to touch on a few details of so extensive a treatise; so it may be well to begin by saying that M. Fougères' work appears to us to have been very well done, combining a considerable amount of research

with the lucidity and order which generally mark French scholarship. The chapter most open to criticism is perhaps that dealing with Mantinean cults, where the writer has fallen into the common error of attempting to explain everything, where much is necessarily fortuitous. An example of this will be found in the paragraphs on the relations of Maira (representing the solar principle), to Poseidon and Demeter, and their respective spheres of influence in the Mantinean plain (pp. 251-3).

Of the historical part of the work (Book III.), perhaps chaps. vii. and viii. are the most interesting. They cover the period from B.C. 387 (Peace of Antalcidas) to 362 (Battle of Mantinea). The former chapter recounts the razing of the city by the Spartan Agesipolis, the exile of the democratic party, and the enforced 'dioecism' (i.e. dissipation of the inhabitants into a number of separate villages); and the latter includes the rebuilding of Mantinea, with its modern and elaborate fortificationwall (still in great part extant), and the Arcadian revival under Theban influence. Among Mantinean institutions (Book II, chap. vii) the most characteristic are the systematic cultivation of music and dancing, for which the town was famous, and the custom of ὁπλομαχίαι and μονομαχίαι. Fougères' theory that the latter, invented by the legislator Demonax in the 6th century B.C., were intended as a mitigation of a preexisting system of vendetta, can hardly be correct. The organised duel would be as repugnant to Greek ideas in historical times as the gladiatorial combat, which Fougères rightly discards. The term μονομαχία clearly denotes, like ὁπλομαχία, a military exercise, though apparently a dangerous one-see Herod. vi. 92, ' μουνομαχίην ἐπασκέων τρεῖς μὲν ἄνδρας τρόπω τοιούτω κτείνει.' Other references will be found in Daremberg and Saglio, s.v. " Hoplomachia."

We must not linger long over the archaeology (in the narrower sense), which occupies Book II, chap. iv and Appendix II. By far the most important work of art discovered in the course of excavation is the series of bas-reliefs (Apollo, Marsyas, and the Muses), which adorned the base of Praxiteles' temple group of Leto and her children. The contemporaneity of these reliefs with the group itself is now very generally admitted. It is curious that Fougères,

while comparing them with the mourners ('Pleureuses') on one of the Sidon sarcophagi (date probably about 370 B.C.), omits all reference to the 'Pyrricist' base in the Acropolis Museum at Athens. The basreliefs on this base contain two figures almost identical, in posture and drapery, with two of the Mantinean Muses; and the date is fixed, by the archon's name, to either 366 or 323 B.C. (see Beulé, Acropole d'Athènes, vol. ii. pl. 4 and 5, and C.I.A. II. 1286). As regards the arrangement of the Mantinean plaques, the author holds to his original theory (Bull. de Corr. Hell., 1888, p. 106) that the base was a square one, with one plaque on the front, one on each of the sides, and possibly a fourth (no longer extant) at the back. His reasons for discarding Dr. Waldstein's proposed arrangement (Americ. Journ. Arch., vol. vii. pl. 1)a continuous frieze of four plaques decorating the front only, the ends and back being plainare based partly on the probable dimensions of the group and partly on aesthetic grounds. Dr. Waldstein's arrangement also necessitates the change of reading in Paus. viii. 9, 1 from Movoa to Movoai, a change which seems to us (though not, apparently, to Fougères-see pp. 544-5) to be unjustifiable. It is very unlikely that, if Movoar had been written, it would have been changed to Movoa, since the Muses are nearly always spoken of collectively, and the plural would therefore have been more familiar to the transcriber than the singular. But it is still possible that a minute examination of the plaques in every detail may decide the much-debated question of arrangement on material rather than on aesthetic or textual

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On topographical questions M. Fougères generally exercises a sound judgment. In his identification of the two roads from Mantinea to Orchomenus M. Fougères (pp. 121-3) defends the generally-accepted theory, as against the contention of the present writer (Journal of Hellenic Studies, vol. xv., pp. 84-5) in favour of the earlier view taken by Col. Leake. He explains the inversion of Pausanias' order of description (E., S., W., N.) by reference to the Temple of Artemis Hymnia, which lay on the westernmost route and was common to the Mantineans and Orchomenians, thus forming a natural transition from the account of Mantinea to that of Orchomenus. The question must still be regarded as an open one. In the case of the so-called 'Πτόλις,' i.e. the site of the original Mantinea, abandoned in historical times,

Fougeres and the present writer are at one. The hill of Gourtzoúli, conspicuous from the later site, is not $\Pi\tau\delta\lambda$ is but the 'Tomb of Penelope,' $\Pi\tau\delta\lambda$ is being represented by a lower hill about a mile further north. The expression $\pi\epsilon\delta\acute{\iota}o\nu$ où $\mu\acute{e}\gamma a$ καὶ $\check{o}\rho$ os $\grave{e}\nu$ $\tau \widehat{\omega}$ $\pi\epsilon\delta\acute{\iota}\omega$ (Paus. viii. 12. 7)—or even the proposed emendation of it, $\pi\epsilon\delta\acute{\iota}o\nu$ καὶ $\check{o}\rho$ os où $\mu\acute{e}\gamma a$ $\grave{e}\nu$ $\tau\widehat{\omega}$ $\pi\epsilon\delta\acute{\iota}\omega$ —is otherwise unintelligible (J.H.S. vol. xv. p. 84).

The great battle of 362 B.C., always known as the battle 'of Mantinea,' is rightly described as taking place just over the border, in Tegean territory, though nearer in point of distance to Mantinea than to Tegea, and fought as an attack on, and in defence of, the former town (cf. Journal of Hellenic Studies, vol. xv. p. 87). And the correct explanation is given of the name $\sum \kappa o \pi \eta$, which is not really derived from, but rather gave rise to, the legend that Epaminondas, when wounded, was carried to this point of vantage to witness the termination of the battle. Remains of a fourth century watch-tower have, in fact, been found on a spur of Mýtika, about 400 feet above the plain, commanding a fine view of the territories both of Mantinea and of Tegea.

Fougères definitely, and very wisely, abandons the tradition supported even by his colleague Bérard (Bull. de Corr. Hell. vol. xvi. p. 534) that the river now known as Sarandapotamós, which at the present day turns eastward after entering the Tegean plain, and disappears down the Katavóthra of Vérzova, formerly flowed westward to that of Taka (whose waters, according to the erroneous theory of Pausanias, reappeared at the springs of Frankóvrysi, and ultimately found their way to the Alpheus). This tradition is clearly disproved by a careful study of the levels and of other natural features of the ground. Our author's counter-theory that, by the excavation of a short and shallow canal to the eastward of Tegea, the river was at times, and for hostile purposes, diverted into the bed of the stream which flows northward towards Mantinea (see Plate IX.), and that the name Aayas (Herodian, Fragm. iii. 26) belongs to this partly artificial stream, is plausible, and even probable, but necessarily incapable of proof.

The map of the Tegean plain, and that of the Mantinean territory and surrounding country, (Plates IX. and X.), well illustrate the topographical portions of the book. In the former, which is specially designed to show the levels of the ground, with reference

to the course of the Sarandapotamós, the current error is repeated of identifying the 'Xωμα,' (Paus. viii. 44-5) which marked the boundary of the Megalopolitan territory as against the Tegean and Pallantian, with the causeway which still exists near the village of Birbáti; an error which has been corrected in Journal of Hellenic Studies, vol. xv. pp. 34, 35 (cf. Frazer's Pausanias, vol. iv. p. 419). The latter-a more general map, but on a smaller scale—is well designed and executed; but the modern parts of it are not quite up to date. For example, of six carriage roads radiating from Tripolitsá, only two are marked; and the new road from the Khan of Plátsas to Kapsiá is omitted, while the old track (not a carriage road) to the same place from the point of Mýtika is shown. The ancient boundaries of the Mantinean territory are very rightly inserted, though necessarily conjectural in part.

We take leave of this conscientious and exhaustive monograph with a hope that, what M. Fougères has done for Mantinea, other scholars may by and by undertake in connection with other Greek cities and territories. Such studies, involving much labour in the library, are a fitting sequel to the more romantic task of excavation and

exploration in Greece itself.

W. LORING.

FURTWÄNGLER'S ARCHAEOLOGICAL PAPERS 1898—1899.

I. Neue Denkmäler Antiker Kunst: von A. FURTWÄNGLER, aus den Sitzungsberichten der philos.-philol. und der histor. Classe der K. bayer. Akad. d. Wiss. 1897. Bd. II. Heft I. 6 m.

II. Zu den Tempeln der Akropolis von Athen, von Adolf Furtwängler: aus den Sitzungsberichten, etc. 1898. Heft III.

III. Griechische Original-Statuen in Venedig, von Adolf Furtwängler (mit 7 Tafeln und mehreren Textbildern): aus den Abhandlungen der K. bayer. Akademie Munich 1898. 5 m.

IV. Neuere F\(\text{ilschungen von Antiken:}\) von ADOLF FURTWANGLER (Giesecke und Devrient, Berlin und Leipzig, 1899). First read as a paper before the Bavarian Academy, November, 1898. 5 m.

V. Ueber Kunstsammlungen in alter und neuer Zeit: Festrede gehalten in der öffentlichen Sitzung der K. bayer. Akademie am 11. März 1899 von Adolf Furtwängler. 1 m.

ACADEMIES may be reckoned among the best abused of institutions. We are all familiar, mostly on the authority of those outside them, with what they have ignored, suppressed, or omitted; and there are obviously many reasons why it might be difficult at the present day to defend institutions which by their nature must be exclusive, aristocratic, and, as a general rule, unpractical. But so long as there are any regions left unsubdued to the common sense of the average man, academies will find room to They serve to employ and to flourish. dignify activities which might otherwise fail to find an outlet in commoner channels. At least will it be instructive to note the results achieved in some fifteen months in one department of the Bavarian Academy by a single savant.

I. The first monograph, quoted above, makes known, with the help of twelve wellexecuted plates, some unpublished objects, of which the most interesting are a bronze head from Sparta and of Spartan workmanship (Museum of Boston) of the period about 460 B.C., an archaic statuette of a youth of the type of 'Apollo,' from Olympia, now in the rich collection of Mr. E. P. Warren at Lewes. The text to three further bronze statuettes of youths, which served as supports for mirrors, constitutes an elaborate disquisition on this class of figures. An analogous female figure of great beauty, from the early years of the fifth century, now in the collection of Mr. Charles Loeser at Florence, is known to the present writer.

II. In the second monograph Professor Furtwängler brings fresh detailed argument to show, versus Professor Dörpfeld, why the 'old temple' discovered on the Akropolis by Dörpfeld and the Greek excavators in 1885 cannot possibly have been left standing after the building of the Erectheion. We note that Furtwängler has again abandoned a view he passingly adopted in the English edition of the *Masterpieces* (p. 425), that the opisthodomos of the inscription (C.I.A. i. 32) was a separate building. He now reverts to his former opinion that it was simply the large back chamber of the Parthenon. In a second article he reverts by way of answering the objections raised to it, to the restoration he proposed in his Intermezzi 1 for the eastern pediment of the Parthenon, maintaining that the Athena Medici is the lost central Athena, a view to which the present writer strongly inclines,

¹ See Class. Rev. 1896, p. 444 ff.

since seeing the cast of the Medici torso, placed in the new museum of casts at the Louvre, in that relation to the Parthenon sculptures which Furtwängler claims for it.

A third article discusses afresh the date of the little temple of Athena Nike: Furtwängler combats the view of Kavvadias that the now famous inscription of the years 460-450 Bc., discovered in 1897, refers to the building of the temple we know. According to Furtwängler, who, relying on the character of its sculptures, still maintains that the temple is contemporary with the Propylaia (i.e. first period of the Peloponnesian war), the inscription must refer to a plan projected but never

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III. In hunting through the Italian museums in search of Roman copies after Greek statues, Professor Furtwängler has had the signal good luck to find not a few Greek originals which had passed unheeded at least by the public. In the monograph before us we have a first instalment of such originals, from the Venetian Museo Archeologico, which has lately been rearranged by Dr. Mariani. These statues, which are well reproduced, are purely Attic works of the fifth century, not perhaps of the highest order of merit as works of art, but pleasantly reflecting the excellencies of their period. Curiously enough they nearly all represent Kora and Demeter, so that Furtwängler conjectures that they probably came as a group from the same 'find spot,' i.e. from some shrine of these goddesses. The beautiful figure on Plate I. is very close indeed to Parthenon work. Owing to this fact as well as to its excellent preservation (the head is original) it should, together with one of its companions, a lovely Demeter with veil and kalathos, soon find its way into our handbooks of Greek art. Our author himself rises to great enthusiasm in describing these figures, wisely forgetting for the moment his own dictum in the preface to the Meisterwerke that, where the great originals are lost, copies after these are better worth one's study than originals by minor masters. Historically perhaps, yes,—but aesthetically, no. Besides this charming group of figures we get a new type of Athena of the Pheidian period also in the Doge's palace, and a very pretty early Praxitelean Artemis with her dog, now in the Museo Civico Correr. In a note Furtwängler states that he has examined the glorious relief of Herakles (not Theseus) in the Doge's palace, probably the only antique in Venice which is familiar to the layman,

and has ascertained that the realistic details of trees and architecture which clash with the Pheidian character of the personages are due to a working up in the Renascence.

IV. This book is the expansion of a paper read before the Academy on November 8th of last year; a notice of Professor Furtwangler's attack on the forged head acquired by the Museum of Berlin appeared, believe, in the Times of Nov. 18th, 1898. As the Berlin management have since then acknowledged the head to be modern and withdrawn it from exhibition, it would be ungracious to dwell further upon their The book contains, however, a error. number of important criteria for the detection of forgeries. These criteria Furtwängler applies to various examples, chief among which are a clumsy copy of a head of the Aeginetan Athena that haunts the Roman market, the Hera of Girgenti in the British Museum and kindred heads with their long noses and weak chins, and a Doryphoros recently acquired for Ny Carlsberg. This collection, indeed, while owning so much that is precious and beautiful, contains, alas, too many examples also of the forger's art. A valuable section deals with forged Roman portraits, and to many it will come as a blow to learn that the popular bust of Caesar in the British Museum (Bernoulli i. Pl. 15) is nothing but a modern forgery.

If forgeries too often pass as antiques, it also sometimes happens that a genuine work which does not fall into any known class has been too readily labelled as a forgery. Professor Furtwängler claims genuine a beautiful bronze female head with turreted crown in the Bibliothèque Nationale, to which he at last gives the publicity it deserves. It had lately been suspected by several French savants, who were misled no doubt by the unusual shape of the turreted crown; I had already ventured in the pages of this review,1 in a notice of M. Reinach's Bronzes de la Gaule Romaine, to assert the genuineness of this bronze, though the date there put forward would, according to Furtwängler who considers the head Augustan, be too early. In dealing with the forgeries of vases and of terra cottas we note that Furtwangler maintains that a vase in the Louvre moulded in the form of two heads back to back is a forgery.2 M. S. Reinach on the other hand in his review of

¹ Class. Rev., 1894, p. 138. ² He already put forth this view in Cosmopolis, 1896, p. 579.

the 'Fälschungen' 1 inclines to defend the authenticity of the vase. I can only say that on careful examination of the vase the breakages certainly seemed to me singularly suspicious, nor is M. Reinach's argument derived from the Hermes of Olympia convincing; the whole head of the Hermes was intact, having lain in a bed of soft earth, while in the Louvre vase what is suspicious is that the actual features are untouched while all the surrounding parts of the heads have been violently broken. Forged bronzes and gems are also discussed at length. Perhaps the only flaw in this illuminating book is the ironical tone adopted towards the Museum of Berlin, who certainly are not as fortunate in their acquisitions as when Furtwängler was the honoured head of the Antiquarium. One could wish also that Furtwängler had allowed to drop, as finally settled, the question of the tiara of Saïtaphernes. I fancy that, even in France, to attack the tiara will soon be looked upon as flogging a dead horse.

V. If the Munich professor can thus, while preparing his huge Corpus of gems, find time to pour out monograph upon monograph dealing with special theories arising from more or less isolated facts, the last paper before us shows him to be equally alert in the realm of general ideas. His paper, which is this spring's annual address at the open or public meeting of the Academy, deserves to be translated and freely circulated; it is not only a learned disquisition on the contents of the temples, the Pinakotheks and treasure-houses of antiquity or an historical essay on the formation of our modern museums; it sketches their possibilities of development into a future which we hope may not be too distant, though to some it will doubtless appear Utopian. The architectural character of the Museum (Furtwängler advocates a simple almost neutral environment for works of art that these may work their full and undisturbed effect upon us), its arrangement, its organisation-larger staffs, whose members shall expound the works of art to the public-its aim, and above all the spirit of science that should reign there are all dwelt upon. So far as architectural reform is concerned, we shall not altogether wish it to be retrospective. No one who in the Louvre has strained his eyes in studying the ill-lit collection of vases, or tried to appreciate correctly, in spite of the cross lights, the modelling of a statue, will wish for

museums built as palaces. We may also regret with Furtwängler the step taken by Vienna, where the palatial plan of the New Museum, with its gorgeous decorations, reduces the works of art exhibited to mere insignificant detail; where, as our author puts it, 'the loud tones of a powerful orchestra drown the silent music of the works of art themselves, which reaches us at the most only as a plaintive melody." Still, although many old collections need wise rearrangement, one cannot deprecate too strongly moving them altogether from their time-hallowed seats. Those will appreciate my meaning who, at Florence, miss the Idolino in the Uffizi, where harmonious surrounding only enhanced the loveliness of the bronze youth. Now we have to look for him in a sort of dismal one-windowed closet in the new Museo Archeologico, whither, if report says true, more of the Uffizi statues are to be ultimately transferred. There is some danger, I have often thought, lest the desire for scientific museums versus the picturesque grouping of antiques put in fashion at the Renascence, become a dangerous weapon in the hands of ignorant and inartistic curators anxious only to enrich some petty museum they govern at the expense of older and wealthier collections. Professor Furtwängler himself shows that the scientific method is not altogether admirable when it results in giving equal relief to the good and to the insignificant relics of the same epoch; such an arrangement is mostly the cause of the ennui so often felt by the general public when they visit museums. Rather, if we understand him rightly, should each period have its 'Salon Carré' or its 'Tribuna' where its best achievements will be grouped together.

Museums in our modern sense of the word were practically unknown to antiquity. The temples of Greece had only accidentally assumed that character, owing to their crowd of votive offerings which were so many precious works of art. Again, when these had made their way to Rome as spoils of war to adorn the public temples and the private villas of the conquerors, the formation and arrangement of galleries were governed by the accidents of conquest rather than by public spirit or scientific intention. True, the great Agrippa had 'urged that all pictures and statues should be made public property,' and Asinius Pollio had thrown open his own galleries to the people; spectari sua monumenta voluit, but these attempts were isolated and therefore abortive.

The idea of a public Museum for the en-

¹ Revue Critique, 1899, p. 246.

joyment and instruction of the people as opposed to the private gallery intended primarily for the pleasure of its owner was reserved for our century and, as Furt-wängler points out, is of Napoleonic origin. It was the genius of Napoleon which planned and realized for the first time in the Louvre, as our own Hazlitt saw it in the year 1806,1 a representative collection of the best art of the world, entrusted indeed to the nation then most powerful to protect it, but generously thrown open to the whole of civilized humanity. The events of 1815 caused the re-dispersion of the Napoleonic Louvre, but the idea fructified, and to it we owe the great representative collections which soon adorned every great capital, and which were thrown open without restraint to the public of all nations. In reviewing the history and the development of these various museums, the German savant pays a high and well-merited tribute to the British Museum, which, as he quotes from the words of the foundation, was established from the beginning 'for the general use and benefit of the public,' and he continues: 'a collection thus supported by the scientific spirit could contain within itself the beautiful or even the most beautiful-and the good fortune which accompanied this foundation, soon brought to it in the sculptures from the Parthenon, the greatest marvel of pure beauty which antiquity has left us—but it could also afford to receive what was outwardly insignificant and mutilated, though to the eye of the scholar priceless; nor did it need to rest content with showy and decorative objects worked up or restored in modern times. The British Museum was a free state-for a long time the only one-for original works of art from Greece and the East which were left unrestored and untouched by any modern hand. Here were soon gathered together all the most significant works, which have enabled us to build up by degrees out of Greek and Oriental originals a history of art such as Winckelmann in Rome scarcely dreamt of.' We cannot pretend to give even a summary of the wealth of ideas and of facts contained in the speech which crowns the year's archaeological contributions of the Bavarian Academy-but we would emphasize the spirit of liberality towards foreign achievements as evidence that the author practises

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The five papers before us are an example of that breadth of knowledge of all the branches of archaeology, which is the only true or sure foundation for our aesthetic enjoyment of ancient art. In the Essay on Museums, Furtwängler deprecates the silly notion that knowledge can destroy or cramp our feeling for art, and he adds in a passage which students of his subject should ponder, 'only knowledge can lead The feeling for to real understanding. art which is based upon understanding will deepen in proportion as the necessary steps to knowledge are conquered, and our outlook becomes freer. Pure realization of the work of art is the art scholar's most precious reward for his labours.'

EUGÉNIE STRONG (née SELLERS).

MONTHLY RECORD.

ITALY.

Rome. The excavations in the Via Giulia for the recovery of the fragments of the Forma Urbis have come to an end; 451 new pieces have been found, making a total, with those found elsewhere, of 1,034. In the Forum, remains have been found of two earlier temples of Saturn, and of an altar, which preceded them. The first temple was built in 497 B.C., and to this belong the remains of a platform of tufa; the second, by L. Munatius Plancus in 42 B.C., to which belong the travertine platform of the existing one and fragments of the antae and architrave of the door. A stone wall at the lowest level may possibly belong to the primitive altar. Six more steps of the front of the temple of Antoninus and Faustina have come to light, showing that the original level of the Via Sacra was about four feet below that of the paved road which dates about A.D. 600.\frac{1}{2}

According to Dionysius of Halicarnassos (i. 87) the site of the grave of Faustulus was marked by a stone lion of archaic workmanship near the Rostra, within the Comitium. Varro, on the other hand, mentions two lions guarding the grave of Romulus in the same corner of the Comitium. The pedestal of one of these lions has now been found, and is clearly the work of an Etruscan stone-cutter; the other pedestal is probably to be found on the opposite side of the 'black stones.' It is four feet below what had been supposed to be the level of early Rome, and measures 6 ft. by 3 ft. Near it was found an archaic bronze statuette of an angur carrying a lituus, with

of the 'black stones.' It is four feet below what had been supposed to be the level of early Rome, and measures 6 ft. by 3 ft. Near it was found an archaic bronze statuette of an augur carrying a lituus, with head bent backwards as if scanning the sky.

The drainage-system of the Via Sacra has been thoroughly explored and cleared. The drains are of three periods: (1) opus quadratum covered with flag-stones, of early Roman date; (2) opus reticulatum of the Augustan age; (3) opus latericium of the Empire. The latter are paved, and covered with stamped tegulae bipedales."

what he preaches for the ideal Museum.

¹ The Louvre, in Notes of a Journey through France and Italy.

¹ Athenaeum, 1 April.

² Athenaeum, 22 April.

GREECE

The excavation of the Stadion has Epidauros. thrown much light on the question of the form and arrangement of the older Greek stadion. Firstly, it had not a semi-circular, but a rectangular termination. Secondly, the starting-line and the winning-line were in Greek times marked simply by a line, and not as later by stone paving. The course was marked out later by stone paving. The course was marked out by small columns every hundred feet, rendering it possible to shorten it if necessary, e.g. for boys' and girls' races. Thirdly, the places of the competitors at the start were marked out by iron pegs, and not, as in Roman times, by half-columns. Fourthly, light has been thrown on the expression of Pausanias, i, 26, 7, $\gamma \bar{\eta} s$ $\chi \bar{\omega} \mu a$. In his time the Stadion had stone seats, as now existing, but the $\gamma \bar{\eta} s$ $\chi \bar{\omega} \mu a$ refers to Stadia which had no stone foundations or walling. It was spoken of as a 'stone' stadium, not because of the stone seats, but because the wooden framework or supports were replaced by stone foundations and the stone seats, but because the wooden framework or supports were replaced by stone toundations and supporting side-walls. The I form of the iron clamps on the seat of the 'Apwoolkaa shows the early date, this form being characteristic of the fifth and fourth centuries, but not of a later period. A base was found inscribed Opacuphys & *molyne. The total length of the stadium has been shown to be about 197 yards.

ASIA MINOR.

Prienc.—A statuette has been discovered which appears to be a portrait of Alexander the Great. It is of rather rough, yet skilful workmanship, and probably dates from the life-time of Alexander, who was a great benefactor to Priene. The marble is that commonly used in works found here. The head is described by Kekulé as having a strongly-modelled lion-like brow, indicating a strong daring will, the eyes imperious in expression, the lips full of disdainful pride.

AFRICA.

Carthage .- Recent excavations have been carried on by Dr. Gaukler, on a part near the shore that was

³ Berl. Phil. Woch., 11 March. ⁴ Ibid, 22 April.

always inhabited. A large shaft was sunk right down to the living rock. The first graves encountered were Byzantine; below these were late Roman buildings, then a Roman house of the time of Constantine containing a marble head of M. Aurelius, and two rectangular rooms with very fine mosaic pavements, meriting a detailed description. The larger measures 12½ ft. by 15½ ft., and represents sea-scapes: about twenty figures boating, fishing, and walking by the sea; Venus adorned with jewellery and holding a mirror, rising out of a large mussel-shell held by two sea-monsters; and on either side, a medallion with the bust of a Triton blowing a conch-shell. The smaller represents mounted hunters with javelins and double-headed axes pursuing lions and panthers. They date from the fourth century after Christ, but are entirely pagan in character. Adjoining these rooms is a large hall with painted stucco decorations rooms is a large hall with painted stucco decorations and a row of niches, with a partition-wall across it. In the further part were found pottery, Christian lamps with the fish, palm, and cross emblems, and fragments of painted stucco in Pompeian style. Among other finds are: statues of Venus with dolphin, seated Jupiter with eagle, Bacchus giving drink to panther, seated youth in chlamys, and head of Cupid; masks of Seilenos and a goddess; a lion's head water-spout: two terracotta figures of Mithras: head water-spout; two terracotta figures of Mithras; part of a statuette with the head of a Carthaginian horse; and a realistic portrait of a woman. In the corner of the hall, fastened to the wall, was a marble plate inscribed IOVI HAMMONI BARBARO SYLVANO with a dedication by the twelve priests of the god and the mater sacrorum; below it was a marble bull's head with crescent and an inscription to Saturn, also twenty baetyli of granite and various votive balls of stone and terracotta, such as have been found before in Carthage. Also a marble statue of a veiled goddess, and a group of Ceres, a Canephoros, and a veiled woman. Below all these were Punic tombs of the sixth century B.C. containing gold and bronze objects, scarabs, etc.⁵

H. B. WALTERS.

⁵ Berl, Phil. Woch, 29 April.

SUMMARIES OF PERIODICALS.

Neue Jahrbücher für das Klassische Altertum, etc. Vol. 3. Part 1, 1899.

Die Geschichtliche Entwickelung der griechischen Taktik, E. Lammert. As democracies superseded oligarchies, the cavalry—the weapon of the aristocracy—came to be of less importance than infantry. So tactics were nearly confined to hoplites. It was Alexander who brought to perfection the tactics of Alexander who brought to perfection the tactics of horse and light-armed troops. Neuestes aus Oxyrhynchos, F. Blass. (1) Fragments of Aristoxenus which B. attributes to his ἡνθμικὰ στοιχεῖα, (2) some lines of a lyric poem which he conjectures to belong to Alkman, (3) lastly a piece which comes from Sappho or more probably from Erinna. Griechische und römische Bildnisse, O. Rossbach. Seeks to show that in two cut stones protraits of Horses and Aris. that in two cut stones portraits of Horace and Aris-tippus are to be recognised. By a comparison with coins the result is that in busts of the so-called Papyrus-villa of Herculaneum the following seven princes

are represented: Ptolemy I. Soter, Ptolemy II. Philadelphus, Ptolemy V. Epiphanes, Antiochus II. Theos, Antiochus IV. Epiphanes, Demetrius I. Soter, Seleucus I. Nikator, and lastly Cassius the murderer of Caesar.

murderer of Caesar.
Part 2. Die Orestessage und die Rechtfertigungsidee, Th. Zielinski. The two new truths of the Apollo-religion in the field of morality were (1) the murdered man has his right to be avenged secured to him, and (2) the avenger is justified by the declaration of the Delphian god. The case of Orestes shows the paramount necessity for the avenging of blood, because the slaughter of a mother puts the case at the highest. Neuere Kommentare zu lateinischen Dichtern, C. Hosius. As examples of recent philology the following works are criticised: Heinze's Lucretius, Book III. Rothstein's Propertius, Sudhaus' Aetna, Palmer's Ovid's Heroides with the Greek translation of Planudes, Vollmer's Silvae of Statius, Langen's Valerius Flaccus, and Francken's

Lucan. He who sees such fruit, he concludes, can set his mind at ease about classical study; it still bears the stamp of immortality in spite of 'Americanismus' and 'Realismus.' Aegyptische Einflüsse im romischen Kaiserreich, E. Kornemann. This influence is considered under four aspects (1) The insertion of the Augustan principate into the Roman constitution and the arrangement of certain dynastic questions in the new monarchy of Rome, (2) the organisation of 'nichtstädtisch' territories and the dispossession of the city of Rome (the local government of Rome was now based on that of Alexandria), (3) the formation of an imperial civil service and a bureaucratic control, (4) the new regulation of the system of taxation and financial control in the

Part 3. Die Orestessage und die Rechtfertigungs-idee (concluded), Th. Zielinski. The triumph of the Delphic oracle would have made Apollo the

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